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CONTENTS

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Observations of Oncometopia orbona *(Fabricius) Migration (Homoptera: Cicadellidae)*

John A. Davidson

Department of Entomology, University of Maryland, College Park, Maryland 20742.

ABSTRACT

The leafhopper *Oncometopia orbona* (Fabricius) was observed migrating from the western shore of the Chesapeake Bay, at Sandypoint State Park, to the eastern shore of the Bay on October 14, 1973.

While on a fishing trip to Sandy Point State Park, Maryland, on October 14, 1973, my daughter, Lynn Davidson, and I made the following observations on *Oncometopia orbona* (Fabricius).

We arrived at the park about 1:00 p.m. The park is on the west side of the Chesapeake Bay just north of the Chesapeake Bay Bridge. It was a sunny day with the temperature about 75°F and wind from the west at 10–15 miles per hour. We parked near the beach beside 2 red maple trees about 30 feet tall. As we walked past the trees toward the water, we noticed what at first appeared to be a swarm of bees flying on the lee side of each tree. On closer inspection they proved to be leafhoppers. Forty specimens were collected and later determined to be *Oncometopia orbona* (Fabricius). This species is often found in collections with the preoccupied name *Oncometopia undata* (Fabricius).

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We left the leafhoppers and walked about 200 yards across open sand to the water and began fishing. As the day wore on, we began noticing these leafhoppers flying about on the sand near the water. Slowly their numbers increased until they were landing in swarms on the lee side of any object near the water's edge, apparently seeking protection from the wind. They were quite annoying and several people left the beach because of their activity.

About 4:00 p.m. the wind subsided and the leafhoppers were seen flying from the water's edge in what looked like loose swarms across the bay. They flew facing the wind, but allowed the wind to carry them eastward.

We left the park about 5:00 p.m. and as we passed the 2 maple trees at the parking lot, we noticed the leafhoppers were gone.

I wish to thank Dr. James P. Kramer, Systematic Entomology Laboratory, U.S.D.A., for the species determination and review of this note.

Varroidae, A New Family of Mites on Honey Bees (Mesostigmata: Acarina)

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ABSTRACT

The mite *Varroa jacobsoni* Oudemans, a parasite of honey bees in Asia, is redescribed and figured; *Euvarroa sinhai*, n. gen., n. sp., also a parasite of honey bees, is described and figured from India; both are from Asia. The family *Varroidae* is erected for the two genera.

The genus *Varroa* was proposed by Oudemans (1904a, b) to accommodate a laelapid-like mite found parasitizing *Apis indica* Fabricius in Java. Although the male was unknown, Oudemans did not hesitate to place this mite in the Laelaptinae because "the female, concerning the dorsal and ventral shields, seems to be nearest to *Hypoaspis myrmemophilus* (Berlese) and *Hypoaspis Canestrini* (Berlese) which are provided too with metapodial as well as with inguinal shields, a rare coincidence; and concerning its being covered dorsally with so numerous hairs,—*Hypoaspis arcualis* (C. L. Koch)." Baker and Wharton (1952) subsequently listed *Varroa* in the subfamily Hypoaspidinae (Laelapidae) and it has not until now been removed from that group.

Upon further examination of the mites infesting honey bees in Southeast Asia we noted that *Varroa jacobsoni* Oudemans (type-species of *Varroa*) showed characteristics that do not fit the present concept of Laelapidae. The discovery of the new genus *Euvarroa* provides us additional support for assigning these mites separate family status under the present classification.

In distinguishing Varroidae from other Mesostigmata, especially the Dermanyssidae-Laelapidae group *sensu* Evans and Till (1965), we considered Oudemans' diagnostic bases for the genus *Varroa* as the major features for characterization of the group, namely the marked modification in the structure of the chelicerae which completely lack the fixed digit, and the number and arrangement of the gnathosomal setae. However, Oudemans (1904b: 216) misinterpreted the cheliceral structure: ". . . that the mandibles in the female sex lack the upper-jaw and have a fixed, not a movable, under-jaw." We are including in this diagnosis other features important in differentiating Varroidae from other groups, especially the palpus and leg chaetotaxy. An interesting feature of the leg and palpus chaetotaxy is the reduction in number of setae compared with those of the Dermanyssidae-Laelapidae group *sensu* Evans and Till (1965).

The material upon which this study is based was received through the courtesy of Dr. A. S. Michael, Bee Laboratory, U. S. Department of Ag-

[1] Published by permission of the Director, New York State Science Service, Journal Series No. 152.

riculture, Beltsville, Maryland, Dr. Donald Johnston, Acarology Laboratory, The Ohio State University, Columbus, Ohio, and Dr. Preston Hunter, Department of Entomology, University of Georgia, Athens, Georgia. The terminology and chaetotaxy used here are those of Baker and Wharton (1952) and Evans and Till (1965).

Family **Varroidae**, n. fam.

Type-genus: *Varroa* Oudemans (1904a b).

The 2 genera included in this family are parasites of wild and domestic honey bees in Southeast Asia, India, Korea, Japan, and the Soviet Far East. They have been collected on dead and live pupae, on adult bees, and in comb-cells and sealed brood.

The females are readily distinguished from other members of the Mesostigmata by the marked modifications in the structure of the chelicerae which completely lack the fixed digit, and by the number and arrangement of the gnathosomal setae. The stigmata and peritreme are situated ventrolateral; the peritreme is short and strongly looped and directed posteriorly or laterally in the region of coxa IV. The tined setae on the palps are 2-pronged, with the basal prong markedly reduced in size. The body is strongly sclerotized with a single dorsal plate covered with a dense pattern of setae. The form of the tritosternum, sternal region and genital-ventral plate shows an affinity to the Laelapidae-Dermanyssidae group, and the arrangement of the gnathosomal setae is the type seen in the Trachytoidea or Uropodoidea. However, the reduction in the number of gnathosomal setae and the structure of the chelicerae are unique; the latter has lost many of the characteristics of the basic dermanyssid type as given by Evans and Till (1966).

Female.—Relatively large, hairy. Chelicerae short, simple; fixed digit completely lacking; movable digit dentate; slender, almost pointed, without setae, evidently developed for piercing and tearing the skin of the hosts. Corniculi slender, tapering distally and blade-like dorsally. Three pairs of gnathosomal setae present, namely: 1 pair at base of gnathosoma and 2 pairs on hypostome. Tectum if visible simple, with smooth anterior margin. Dorsal plate entire, covering entire dorsum, with narrow thickened margin and ornamented with polygonal network of simple lines or striations and covered with dense pattern of setae which may be simple or barbed, the marginal setae distinctly differing in size from discal setae. Tritosternum if present weak, bifurcate. Sternal plate well developed and bearing 3 pairs of setae, except in *Varroa* which has 5 or 6 pairs. Genital plate with more than 10 setae, the posterior extension approaching anterior margin of ventrianal plate. Anal plate bearing 3 setae; anus terminal. Peritremal plate absent, or possibly fused with podal plates. Podal plates (Evans and Till, 1965) well developed, half circling posterior border of coxae IV; metapodal plates present and markedly developed in *Varroa*. Stigmata and peritreme ventrolateral in position; peritreme strongly looped and extending laterally or posteriorly from stigmata. Exopodal plates curiously developed in *Varroa* but weak in *Euvarroa*. Legs strongly formed; II-IV 7-segmented, and with metatarsus; claws if present not well developed.

Genus **Varroa** Oudemans

Varroa Oudemans, 1904a (July 1), Entomol. Ber. (Amst.) 18: 161; 1904b (July), Notes Leyden Mus. 24: 216.

Type-species: *Varroa jacobsoni* Oudemans, 1904; by monotypy.

This remarkable genus with 1 species (the type *V. jacobsoni*) has the following features in the female: 3 pairs of gnathosomal setae; deutosternal groove smooth, lacking denticles; exopodal plates completely fused with each other anteriorly; podal or endopodal and metapodal plates markedly developed, the latter densely setate; ventral plate covered with dense pattern of setae; sternal plate with 5–6 pairs of setae and 4–5 pairs of pores; stigmata and peritreme ventro-lateral. Peritreme strongly looped and directed laterad of stigmata; peritremal plates lacking or presumably fused with podal plates; dorsal plate entire with a narrow thickened margin, covered with dense smooth and finely barbed setae; legs II-IV 7-segmented. The male is similar to the female but poorly sclerotized; the spermatodactyl is absent and it is possible that the

modified strongly grooved chelicerae function as spermatophore bearers.

Varroa is placed close to *Euvarroa*, n. gen. by the structure of the chelicerae, by the number and arrangement of gnathosomal setae and by the lack of peritremal plates. The main differences between the genera appear to be the presence of denticles in the deutosternal groove and the absence of sternal pores in *Euvarroa*. However, there are other differences, especially in the palpus and leg chaetotaxy which will be discussed in the species descriptions.

Varroa jacobsoni Oudemans

Varroa jacobsoni Oudemans, 1904a (July 1), Entomol. Ber. (Amst.) 18: 161; 1904b (July), Notes Leyden Mus. 24: 216 (as *jacobsonii*). Type-loc.: Samarang, Java, on *Apis indica* Fabricius.

Varroa ricinus Oudemans, 1904c, Entomol. Ber. (Amst.) 19: 169. *Nomen nudum*.

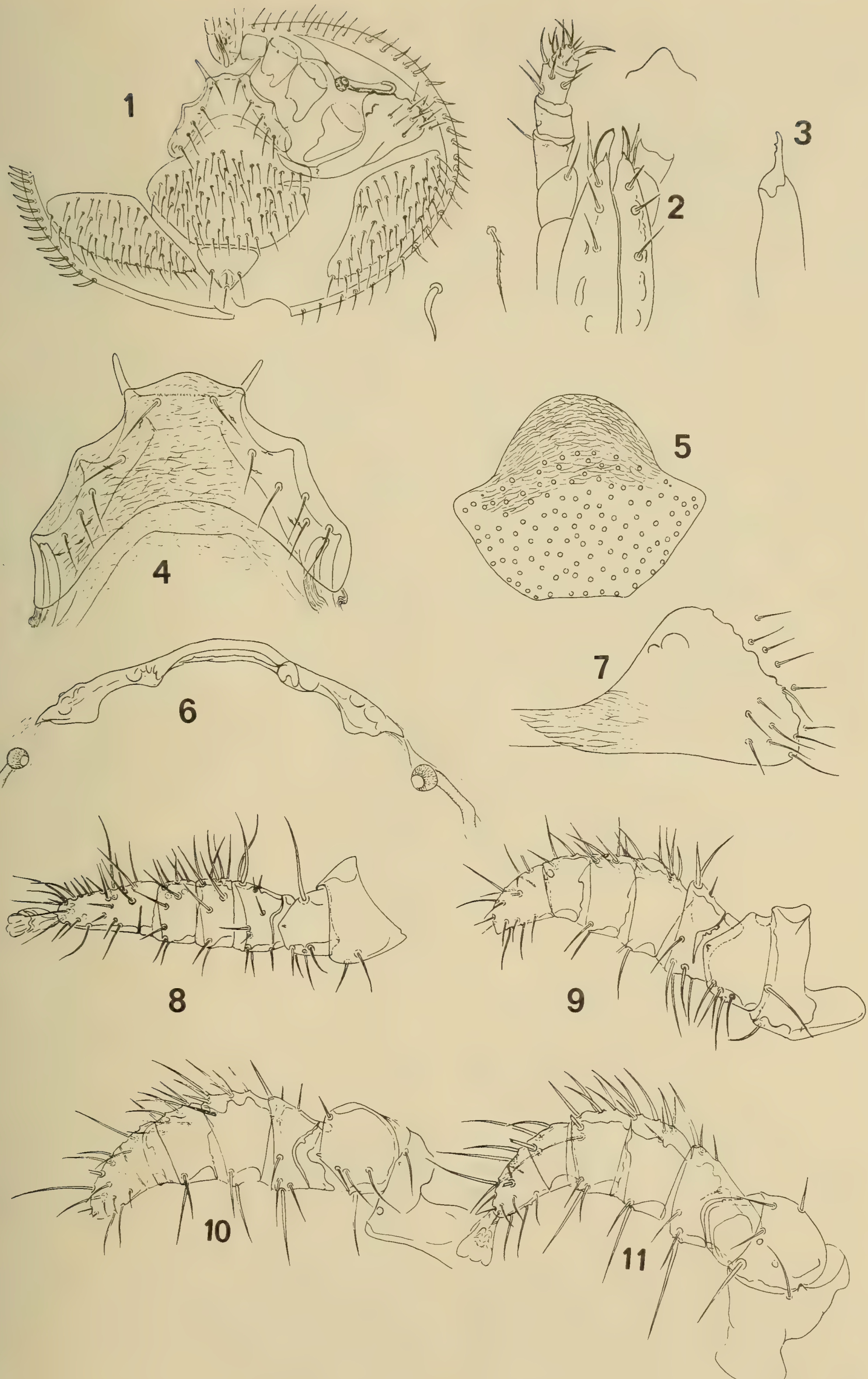
Myrmozercon reidi Gunther, 1961, Proc. Linn. Soc. N.S.W. 76: 155. Type-loc.: Singapore, Malaya, on *Apis indica* Fabricius; Delfinado, 1963, J. Apic. Res. 2: 113. Synonymy.

Female.—Large, brown, hairy species; body broadly elliptical, dorsum completely covered by sclerotized plate; surface of dorsal plate covered with numerous finely barbed setae of varying lengths, discal setae shorter than posterior and lateral setae; ornamented with striations and polygonal network of simple lines; a narrow thickened margin broadened antero-laterally, dorsally bearing short setae, 21–23 stout lanceolate setae at each side as figured. Gnathosoma small, completely hidden beneath dorsal plate, lying between coxae I. Cheliceral bases relatively short, slender; chelicerae short, fixed digit absent, movable digit slender, tapered distally and bearing 2 small teeth, without setae, as figured. Deutosternal groove smooth, without denticle. Tectum as figured, smooth anteriorly. Corniculi blade-like, appearing flat in profile. Salivary stylets present, hyaline and tapering. Three pairs of gnathosomal setae arranged in longitudinal row; 2 pairs of short setae on hypostome, a pair at base of gnathosoma slightly longer than hypostomals. Palpus 6-segmented; tarsal tined seta with short, small basal prong. Palpal chaetotaxy formula as follows: the figures represent trochanter, femur, genu, tibia and tarsus: 1, 2–3, 2, 7–8, 12. Number of setae on femur and tibia variable. Sternal plate with strong internal ridges, extending from anterior margin of coxae II to coxae IV; very long anterior protuberance between coxae I and II; 5–6 pairs of sternal setae and 4–5 pairs of pores present; 3 pairs of pores situated between setae 1 and 2 (pore 1), 2 and 3 (pore 2), 3 and 4 (pore 3), pore 4 laterad or below seta 5. Metasternal plate obviously fused with sternal plate. Genital opening located below sternal plate and difficult to see unless specimens dissected. Ventral plate wider than long, uniformly sclerotized, covered with numerous setae (over 100) except anterior portion; posterior extension straight and approaching anterior margin of anal plate. Anal plate small, bearing 3 setae; paranals situated on each side of anus; with 1 post anal. Anus terminal. Metapodal plates conspicuous, subtriangular and very large, occupying the greater portion of area below lateral plates, completely covered with setae. Endopodal plates (Evans and Till, 1965) greatly enlarged, arising ventrally from posterior region of sternal plate half circling posterior margin of and extending laterally beyond region of coxae IV; lateral portion greatly expanded with strong ridges or protuberances and with 5–8 setae; adjacent integument with 5–8 setae. Exopodals strong, completely fused with each other anteriorly and forming a framework along anterior border of dorsal plate, enclosing the cavities between coxae I and II, II and IV. This structure is best seen in dissected specimens, as figured. Stigmata and peritreme ventro-lateral, stigmata located in region of coxae III and IV; peritreme short, strongly looped distally, extending posteriorly from stigmata in region of coxae IV. Legs robust, 7-segmented with metatarsi present on II-IV although tarsi I show slight evidence of secondary division. Claws do not appear developed. Leg chaetotaxy formula as follows: the figures represent coxa, trochanter, femur, genu, tibia, metatarsus and tarsus. Number of setae on genu, tibia and tarsus of leg IV somewhat variable.

Leg I — 2, 5, 10, 12, 13, 0, 34
Leg II — 2, 5, 8, 11, 11, 4, 13
Leg III — 2, 5, 8, 11, 11, 4, 13
Leg IV — 1, 6, 7, 9–11, 10–11, 4, 12–13

Body length 1135 μ; width 1666 μ.

Male.—Gnathosoma as in female; deutosternal groove without teeth; with 3 pairs of setae in longitudinal line; cheliceral bases as in female, fixed chelae lacking, movable chela broadly tapered to blunt tip and deeply grooved its entire length; corniculi blade-like as in female; palpal tine claw with tiny basal prong. Tritosternum weakly sclerotized, bifurcate. Genital opening anterior; sternal plate weakly sclerotized, with 5 pairs of setae; venter of plate posterior to coxae IV area with numerous short setae. Stigmata and peritreme ventral-lateral, situated between coxae III and IV and directed anteriorly. Endopodal plates present, weak, half circling coxae IV; other plates not or barely discernable; anal plate with 3 pairs of setae. Dorsal plate weakly sclerotized, striated, covered with numerous simple setae, the posterior marginal setae being stronger than others but not as much as in female. Body oval, broadest at coxae IV area. Legs robust, coxae I-IV contiguous as in female, tarsal claws not developed. Body length 800 μ; width 706 μ.



Varroa jacobsoni Oudemans. Fig. 1, venter of female with details of dorsal setae; fig. 2, venter of gnathosoma with tectum; fig. 3, chelicera; fig. 4, sternal plate; fig. 5, genital plate; fig. 6, exopodal plate; fig. 7, endopodal plate; fig. 8, leg I; fig. 9, leg II; fig. 10, leg III; fig. 11, leg IV.

V. jacobsoni has been found parasitizing *Apis indica* Fabricius, *Apis mellifera* Linnnaeus and *Apis cerana javana* Enderlein in Southeast Asia, India and the Soviet Far East; males were recently found in Swon, Korea by K. S. Woo on *Apis mellifera* (Crane, 1968; Delfinado, 1963; Ehara, 1968; Gupta, 1967; Kshirsagar, 1967; Kulikov, 1965; Stephen, 1968; Yoshikawa and Ohgushi, 1965). Kulikov (1955) cited a number of interesting observations concerning the biology and behavior of this mite on bees. Although the mites freely occur on the back surface and under the wings of the bees, feeding takes place on the abdomen "with their mouthparts in the area of the intersegmental membranes whereby the anterior part of the body is hidden below the chitin segment of the bees' abdomen" (translation from the Russian). Apparently the mites suck tissue fluid or haemolymph of the host, but Kulikov suggested that the mites possibly also feed on the regurgitated content of the honey sac. He further observed that *jacobsoni* is viviparous. It is possible that they give birth to first stage nymphs. But we have not observed any intrauterine development in the specimens on hand. The mites chiefly affect drone brood, and up to 5000 individuals may be found in one colony.

Genus **Euvarroa**, n. gen.

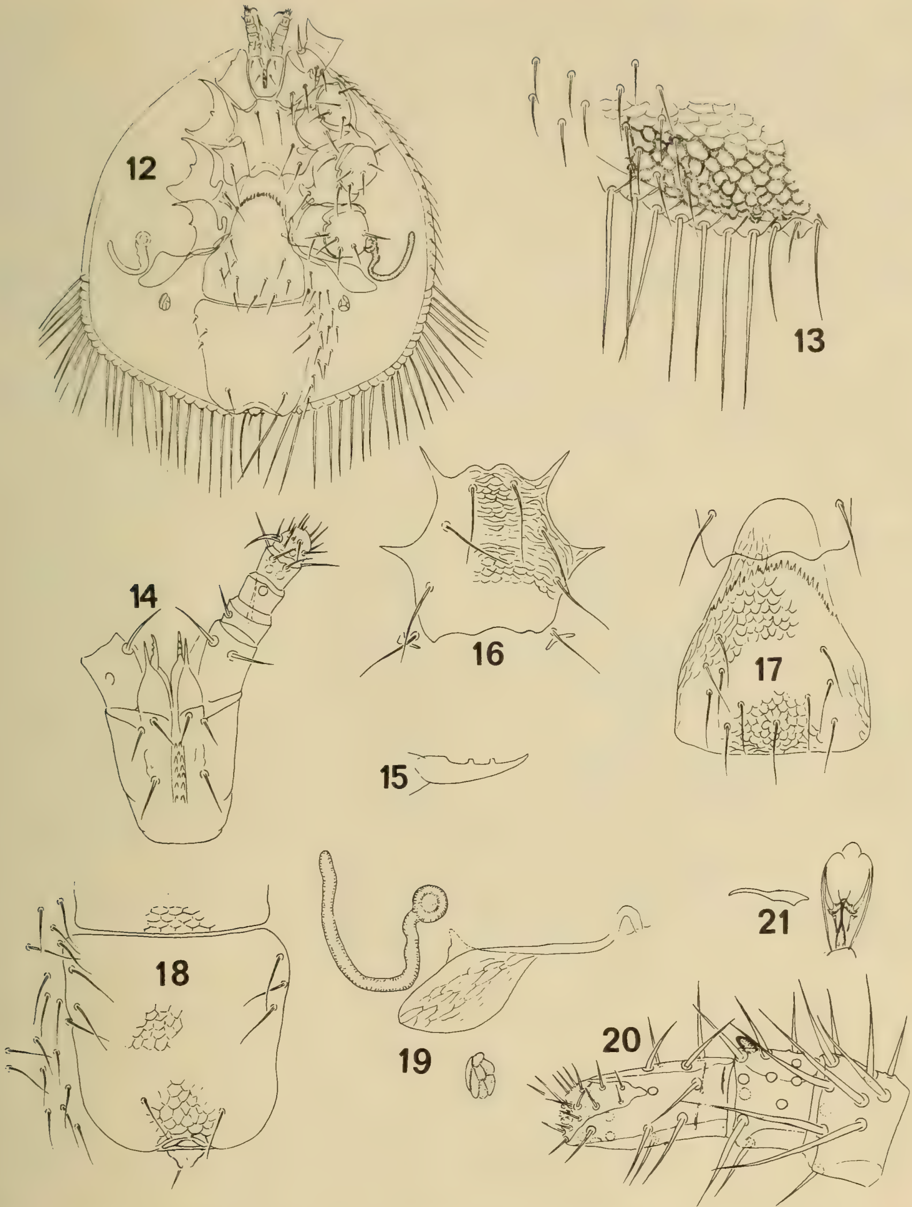
Type-species: *Euvarroa sinhai,* n. sp.

This new genus is similar to *Varroa* by the structure of the chelicerae which lack the fixed digit, by possessing three pairs of gnathosomal setae and by the form and location of the stigmata and peritreme. It obviously differs in that the deutosternal groove possesses 13–14 small triangular denticles.

Female.—Dorsal plate covered with moderately dense simple setae of nearly uniform length, ornamented with reticulate pattern; a narrow lightly thickened margin, broader posteriorly, bearing 39–40 very long lanceolate setae; sternal plate rectangular with thickened anterior and lateral margins, with three pairs of setae lacking associated pores; a pair of metasternal setae lies outside plate just above remnants of the metasternal plates; metapodal plates small, rounded; podal plates half circle posterior border of coxae IV; exopodal plates very lightly sclerotized. Stigmata and peritreme ventro-lateral, stigmata situated adjacent to coxae IV, peritreme strongly looped and directed laterad of stigmata in region of coxae IV. Peritremal plates absent or possibly fused with podal plates. Legs well developed, with stout setae, II-IV 7-segmented; claws simple, developed. Laciniae and tritosternum not seen, probably absent. Forked tine of palpus possesses a tiny basal prong. Palpal and leg chaetotaxy formula as given for species.

Euvarroa sinhai, n. sp.

Female.—Moderately large, brown setaceous mite, body broadly pear-shaped, dorsum completely covered by single sclerotized entire plate; surface of dorsal plate covered with moderately dense simple setae of almost uniform length, ornamented with reticulate pattern, a lightly thickened margin present, broader posteriorly and bearing 39–40 very long lanceolate setae dorsally on individual protuberances as figured; other marginal setae small and dorsal. Gnathosoma small, completely hidden below dorsal plate. Cheliceral bases short; chelicerae short, modified as in *V. jacobsoni,* fixed digit completely lacking; movable digit with 2 large teeth and tapering to pointed tip. Corniculi short, blade-like, salivary stylets well developed. Deutosternal groove with 2–3 uneven rows of small triangular denticles. Three pairs of gnathosomal setae present, with hypostomal setae 2 outside line between hypostomal 1 and gnathosomal setae as figured (the position of 2 may vary); gnathosomal setae longer than hypostomals. Tritosternum not seen. Tectum not seen. Palpal tined seta with tiny basal prong as figured. Palpal chaetotaxy formula as follows, the figures representing trochanter, femur, genu, tibia and tarsus: 2, 2–3, 1–2, 5, 12; setal numbers may be variable. Sternal plate rectangular, extending from posterior margin of coxae I to coxae III, with strong protuberances between cavities of coxae I and II and III, with thickened anterior and lateral margins, with 3 pairs of sternal setae lacking associated pores; a pair of metasternal setae lies outside sternal plate just above remnants of metasternal plate between region of coxae III and IV; associated metasternal pore also missing. Genital plate large, pear- or flask-shaped with straight posterior margin approaching anterior margin of anal plate, bearing 9–10 setae, uniformly sclerotized except anterior portion as figured. Anal plate large, larger than ventral plate, squarish with shallow median indentation at posterior margin; 2 paranals longer and stronger than post anal seta. Anus terminal, hyaline or membranous. About 26 pairs of setae on integument adjacent to anal and ventral plates, 2 or 3 pairs of setae may be situated on anal plate. Metapodal plates small, rounded, without setae. Podal plates



Euvarroa sinhai, n. sp. Fig. 12, venter of female; fig. 13, details of posterior dorsal margin; fig. 14, venter of gnathosoma; fig. 15, chelicera; fig. 16, sternal plate; fig. 17, genital plate; fig. 18, anal plate; fig. 19, endopodal plate, peritreme and small metapodal plate; fig. 20, tarsus, tibia and genu of leg I; fig. 21, ambulacra of leg I.

small, subtriangular, half circling posterior border of coxae IV, without setae. Stigmata and peritreme ventro-lateral; stigmata situated adjacent to coxae IV; peritreme strongly looped and directed laterad of stigmata in region of coxa IV. Peritremal plates absent, probably fused with podal plates. Exopodal plates very lightly sclerotized, enclosing cavities of coxae I-IV and continuing anteriorly as in *V. jacobsoni*. Legs short, strong, II-IV more robust than I; coxal and dorsal setae stronger and stouter than others; legs II-IV 7-segmented, tibia I with a short, strong antero-dorsal spur; claws developed, simple. Leg chaetotaxy formula as follows: the figures represent coxa, trochanter, femur, genu, tibia, metatarsus and tarsus. Compared with *V. jacobsoni* the

chaetotaxy of *E. sinhai* shows a reduction in number of setae in trochanters I-III and femur and genu IV. This overall deficiency in leg chaetotaxy of both *E. sinhai* and *V. jacobsoni* is an interesting feature not well known in the Dermanyssidae-Laelapidae group.

Leg I — 2, 3, 10–12, 11, 12–13, 0, 37
Leg II — 2, 3, 10, 10, 10, 3, 12
Leg III — 2, 4, 5, 10, 10, 3, 12
Leg IV — 1, 6, 4, 8, 10, 3, 12

Body length 1040 μ, width 1000 μ.

Male.—Unknown.

Holotype.—Female, U. S. National Museum No. 3580, taken from colony of *Apis florea* Fabricius, New Delhi, India, August 28, 1971, by R. B. P. Sinha.

Paratypes.—Nine females in the U. S. National Museum collection; the Acarology Laboratory, Ohio State University, and the New York State Museum and Science Service, Albany, New York; all with the data of the holotype.

This species is named for Dr. R. B. P. Sinha of New Delhi, India.

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A Gynandromorphic Specimen of the Genus **Limnia** *(Diptera: Sciomyzidae)*

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ABSTRACT

A serial gynandromorph of a species of *Limnia* (segments 8 and following, female; otherwise male) is described and figured.

Among specimens of the genus *Limnia* being examined for a revision of the genus, Lloyd V. Knutson found a most interesting gynandromorphic specimen, which, because of its nature, could be determined only as far as a group of species including *L. fitchi* Steyskal, *L. ottawensis* Melander, and a few less common species. The tip of the abdomen is here figured (Fig. 1a), together with the tip of the abdomen of a normal female (Fig. 1b) for comparison. The abnormal specimen is basically male, with the abdomen modified as is normal in males of the genus. The 1st 5 segments are essentially as in normal males. The following 2 segments (6 and 7, protandrium) are also much as in normal specimens. The ultimate segments (8th and following) are very abnormal. The sterna of segments 6 and 7, as in normal males, are greatly reduced. Tergum 6, also as in normal males, is virtually absent, but tergum 7 is well developed. Tergum 8 (epandrium) is dome-like as in normal males, but the hypandrium is not evident. Perhaps a flap (f) in the membrane mesad of tergum 7 and attached only at its caudal end represents sternum 8 (hypandrium). A plate that may correspond at least to part of sternum 8 of a normal female subtends segments 9 and a pair of cerci, which are very similar to those parts of a normal female (b).

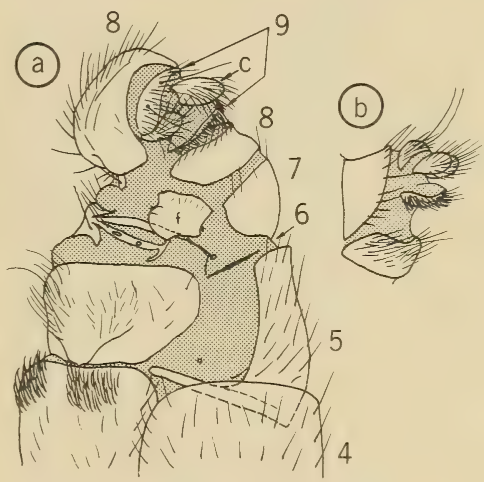


Fig. 1. *Limnia* sp., terminal segments of abdomen in oblique ventral view. a, gynandromorph; b, normal female. c = cercus; f = flap (possibly analogous to male hypandrium); numbers refer to abdominal segments.

The specimen, captured at Breckenridge, Ontario, 26 June 1959, by C. H. Mann, has been returned to the Canadian National Collection, Ottawa.

Biology and Larval Description of Procecidocharoides penelope *(Osten Sacken) (Diptera: Tephritidae)*

W. Bryan Stoltzfus

Department of Zoology and Entomology, Iowa State University, Ames, Iowa 50010

ABSTRACT

The biology of *Procecidocharoides penelope* (Osten Sacken) is discussed, including its seasonal and geographical distribution and the relationship to its host plant *Eupatorium rugosum*. The mature third-instar larva and puparium are described and illustrated.

Procecidocharoides penelope (Osten Sacken, 1877) is seldom collected, but like many other tephritids it can be collected readily if a large stand of its host plant can be found. In the summer of 1972 a series of this species was collected from *Eupatorium rugosum* Houttuyn. This is the first species in this genus for which a host plant has been reported.

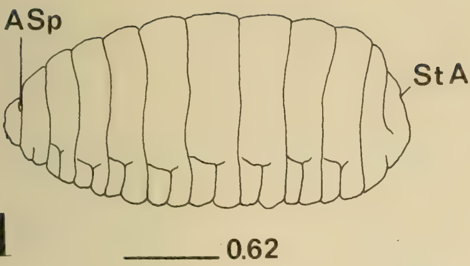
The genus *Procecidocharoides* was erected by Foote (1960) for *Procecidochares penelope* and 3 newly described species. He included a key to genera, wing illustrations and descriptions for all 4 species in the genus.

Biology.—The distribution of *P. penelope* as given by Foote (1965) includes Massachusetts, New Jersey, New York, Pennsylvania, Ohio and Michigan. It has also been taken from various localities in Iowa. Its host plant, *Eupatorium rugosum*, or white snake root, is known from New Brunswick to Saskatchewan, south to Georgia and Texas (Gleason, 1968), where it is found in rich woods and thickets.

P. penelope adults occur from June 25 in Iowa to August 26 as reported in Pennsylvania (Foote, 1960). Peak populations seem to be reached around the first of August.

The larva develops in the heads of *E. rugosum*, destroying nearly all of the 12–24 developing achenes. The flower head of this plant is only approximately 5 mm. in height, and it is surprising that the larva is able to obtain sufficient food in one flower head to mature. No enlargement of the flower head due to infestation was noticed, and no external disfiguration of the head gave any indication of the presence of the larva, at least in the earlier stages of development. Only 2 larvae were taken in 200 heads examined. Larvae were fully developed by mid-September and were leaving the flower heads. By the first week of October no larvae were found in the flower heads. Empty larval sites were found, indicating that the larvae had left the heads to pupate, presumably in the ground.

The method for obtaining reared specimens was to place large numbers of flower heads in plastic bags during September. The larvae would leave the heads as the flowers dried out and could be obtained from the bottom of the plastic bag.



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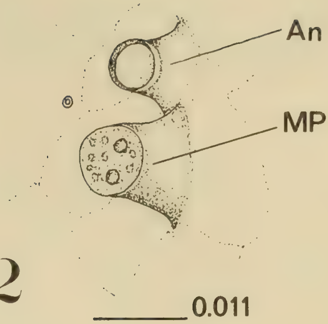

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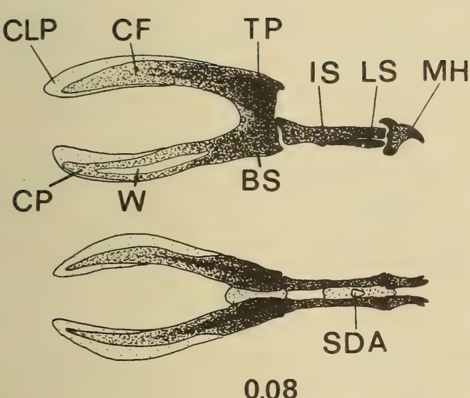

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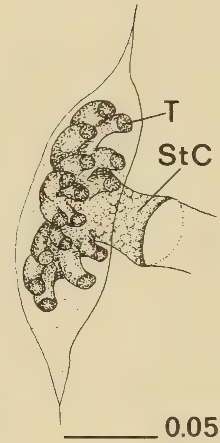

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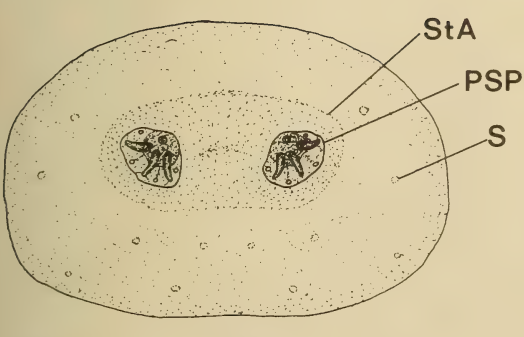

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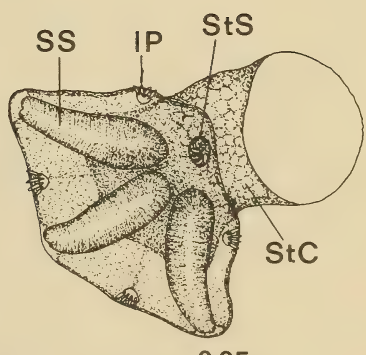

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Procecidochares penelope (O. S.): Fig. 1, lateral view of larva; fig. 2, larval antenna and maxillary palp; fig. 3, cephalopharyngeal skeleton; fig. 4, anterior spiracle; fig. 5, caudal segment; fig. 6, posterior spiracular plate (line lengths expressed in mm).

The larvae of *P. penelope* have no spinules and probably are unable to burrow very deeply in the soil to pupate. This lack of spinules is rather rare among tephritids. Of the 45 species described by Phillips (1946), only 2 species, *Procecidochares atra* (Loew) and *P. australis* Aldrich, lack spinules. Both of these species form galls and pupate within them. However, *Procecidocharoides penelope* pupates in the soil. Perhaps the lack of spinules suggests that it once formed galls in which it pupated but now has found a new host plant on which it is unable to produce galls.

Larval Description.—The terminology used in describing the cephalopharyngeal skeleton follows Roberts (1971). Terms used for other larval structures are after Phillips (1946).

Third-instar larva (Fig. 1): length 3.68 mm, width 1.98 mm, light yellow to white. Head region tapering anteriorly, body truncate posteriorly. Twelve to 20 sensilla around each thoracic and abdominal segment. Spinules absent.

Anterior spiracle (Fig. 4): dark yellow, with 3 segments; dorsal and ventral segments subequal, bearing 6 tubules each; shorter middle segment with 4 tubules. Stigmatic chamber indistinctly reticulated, about 8 cells wide and 7 cells long; cells ending before base of tubules.

Antenna (Fig. 2): width 0.011 mm, height equal to width, with a distinct rim distally, less than its width from maxillary palp.

Maxillary palp (Fig. 2): width 0.012 mm, height equal to width, with 2 larger pegs and several smaller pegs set within a distal rim.

Cephalopharyngeal skeleton (Fig. 3): length 0.33 mm, sclerotized dark brown except marginal areas of clypeofrontal phragma and cibarial phragma. Each mandible with 2 teeth, strongly arched dorsally, with pointed tips. Lateral teeth nearly as long as medials. Intermediate sclerite enlarged distally and mesally. Labial sclerite slender, length 0.14 mm, appearing spindle-shaped laterally but plate-like dorsally and bearing a triangular-shaped salivary duct aperture. Sclerite connecting tentorial phragmata ventrally lightly sclerotized. Basal sclerite dark, with a long narrow window posteroventrally. Clypeofrontal phragma dark anteriorly, with a hyaline margin distally.

Caudal segment (Fig. 5): shagreened, similar to rest of larva, with 16 small tubercles. Stigmatic area with no tubercles, slightly darker than surrounding area.

Posterior spiracular plate (Fig. 6): width 0.18 mm, with four small, distinct, unsclerotized triangular areas near outer edge, each bearing 4 indistinct interspiracular processes. With 3 spiracular slits; outer slits of one plate at 95° to each other. Inner slits of opposing plates nearly parallel, outer slits pointing directly away from each other. Spiracular slits 0.34 mm long, wider medially. Indistinct trabeculae extend from sides of slits, obscuring most of opening. Stigmatic chamber dark; meshes small, inconspicuous and numerous.

Puparium: length 3.25 mm, width 1.7 mm. Black, barrel-shaped. Integument similar to third-instar larva. Anterior spiracles with tubules projecting anteriorly. Posterior spiracular slits easily visible, similar to third larval instar.

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Phillips, Venia T. 1946. The biology and identification of trypetid larvae (Diptera: Trypetidae). Amer. Entomol. Soc., Mem. No. 12, 161 p., 16 pl.

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American Entomologists by A. Mallis. Rutgers University Press, New Brunswick, N.J., 1971, xvii + 551 pp. $15.00.

Though Europe is the cradle of insect science, New World entomologists have had a lion's share in its development. A few outstanding students were working in the Americas during the eighteenth century, while the nineteenth century produced dozens or perhaps hundreds of entomologists of high reputation who have positively influenced the progress of the science. The American entomologists must be particularly credited with the development of economic —mainly agricultural—entomology. European countries were much slower in recognizing the importance of the study of insects for national economies than some of the American countries. In the United States, the first federal entomologist and the first state entomologist were appointed as early as 1854; and from that time on economic entomology has been given much support. It is necessary to say, however, that the Americas also produced many top specialists in all branches of theoretical entomology.

In his book on American entomologists, Arnold Mallis brings to life the foremost of these students of insects. He has not written a history of American entomology but rather a collection of life histories of those diverse men whom we normally know only as author names of learned papers. Mallis does not represent an account of the scientific accomplishments of each of them but rather gives us a picture of each person's character, way of life, interests, abilities as well as eccentricities, friendships and associations with other scientists, personal victories and unfortunate events. Every personality is vivified before our eyes, with his good and bad features highlighted. Displaying a deep understanding of each of them, Dr. Mallis succeeds in presenting a voluminous book replete with facts, which nevertheless remains as thrilling to read as good detective fiction.

From the dozens of life histories of entomologists included in the book (only deceased individuals who worked in the Americas, and immigrants, are included) certain general conclusions may be drawn. First, it seems that a valid assumption could be made to the effect that top entomologists as teachers produce students who also become outstanding entomologists. Simple transmission of knowledge alone is not enough; of equal importance is learning the style of work, the scientific appreciation of exactness, inventiveness, initiative and enthusiasm. Second, success in the science of insects does not necessarily arise from basic talent and good working conditions, but is rather the result of tenacity, good friends and good advisers, and particularly of enthusiasm and devotion. Strong personal involvement has always been the most conspicuous feature of the majority of successful entomologists, whether in the Americas or elsewhere.

The author of the book has a fine appreciation of the variations in human character, and of both the outstanding qualities and amusing traits of many of the well-known entomologists. He has a good sense of humour, and at the same time is well able to appraise seriously the contribution of every individual to entomology and society. The book is not merely interesting and engaging but also represents a part of that general background information which every entomologist, whether in America or elsewhere, should possess or at least be aware of.

J. Zuska

Environmental Management: Planning for Traffic by J. Antoniou. Chartered Architect & Town Planner, Doxiadis Associates, Athens, Greece. 200 pages plus index; 260 illustrations; 11¾ × 8¼; McGraw-Hill; $19.50.

This book examines the elements of environmental management and their practical application to show the effects of such techniques in Great Britain and the United States. First published in England and now published in the United States by McGraw-Hill, it studies ways to reorganize internal urban traffic flow so that it is less damaging to the life of the city.

Antoniou defines environmental management as the method for protecting an area from the adverse effects of motor traffic, using measures designed to prevent the entry of extraneous traffic. He draws examples of existing problems and the methods for dealing with them from numerous European and American towns and cities.

Divided into five chapters, *Environmental Management* first discusses present traffic problems and the limitations of current solutions. The following chapters investigate long-term and short-term solutions and improvements; pedestrian access; traffic pollution; public transport; and the techniques for change. Established principles are applied to practical case studies in several examples to show what can be accomplished to improve the quality of the environment by controlling vehicular traffic.

Concepts in Architectural Acoustics by M. David Egan, 200 pages; 243 illustrations; 8½ × 11; $16.50; McGraw-Hill.

A comprehensive source of practical step-by-step examples and design procedures, this book is based on a new highly illustrated approach to the presentation to acoustics. By means of illustrations which are often both humorous and informative, the book analyzes those concepts vital to the understanding of sound behavior in the environment as concisely as possible. On the premise that the goal of architectural acoustics is to make the environment best serve the functions intended, the text briefly reviews basic theory before proceeding with chapters on sound absorption, sound isolation, speech privacy, mechanical system noise and vibrations, room acoustics, and sound-reinforcing systems.

Head of his own consulting firm in South Carolina, Mr. Egan is an Associate Professor in the College of Architecture at Clemson University and Visiting Professor at the Georgia Institute of Technology. His business career has included positions at the Shell Oil Company and on the consulting staff of Bolt, Beranek and Newman, Inc., where he worked on the design and engineering of a wide range of projects in architectural acoustics and noise control.

Handbook of Precision Engineering, edited by A. Davidson. Vol. 3: *Fabrication of Non-Metals*. 270 pages; 6 × 9; $13.50; Vol. 4: *Physical and Chemical Fabrication Techniques*. 166 pages; 6 × 9; $12.50; Volume 5: *Joining Techniques*. 297 pages; 6 × 9, $14.50; Volume 6: *Mechanical Design Applications*. 320 pages; 6 × 9, $14.50.

Originally published in England, this series deals with all aspects of the design and manufacture of close tolerance devices and mechanisms such as telecommunications equipment; cameras; calculating machines; electric shavers; and electronic equipment.

Volume 3 covers both the traditional and newly developed techniques in the rapidly growing area of working and shaping non-metallic materials. Comprehensive information is provided on such materials as plastics, glass, ceramics, and monocrystalline materials.

Volume 4 presents the physical and chemical techniques that have become increasingly important in the fabrication of precision components, especially in microminiaturization and in the construction of integrated circuits. These

techniques range from electrical discharges, ultrasonics, and thin-film technology to etching processes for a wide range of materials.

Volume 5 describes the numerous methods available for permanent joining, ranging from mechanical joining, welding and soldering, to gluing, winding and braiding. A special section deals with the techniques of application of printed texts onto a variety of materials.

Volume 6 examines the ways that mechanical design is applied in engineering a precision product. A study of the construction elements found in a variety of precision parts and components is followed by a number of practical applications. Finally, the basic elements for the construction of optical apparatus are discussed.

Individual chapters in each of the 13 volumes which will constitute Handbook of Precision Engineering have been contributed by experts drawn from the Philips Organization working under the general editorship of A. Davidson. Subsequent volumes in the series will be published two at a time at six-month intervals.

Through the Crust of the Earth by Lord Energlyn, McGraw-Hill, $10.95.

The author, a brilliant scientist, traces the evolution of earth science and shows how man is linked to geology and geology to man. He explains the ways in which volcanoes spout energy, how the threshing of subterranean masses produces earthquakes, and describes the processes that formed our planet; he reveals new discoveries and theories made directly from the rocks that tell us basic things we neither knew nor could explain before. These discoveries, Lord Energlyn believes, may be extremely significant to our survival.

Lord Energlyn, professor and head of the department of geology at the University of Nottingham, England, discovered the first antibiotic to be found in coal. He advises governments around the world on geological matters and acts as a consultant to various major mining and chemical companies.

Lost Discoveries—The Forgotten Science of the Ancient World by Colin A. Ronan, McGraw-Hill, $10.95.

Amazing discoveries of the distant past—an unexpected blooming of scientific achievements which were subsequently lost, destroyed or suppressed and were not rediscovered for many centuries—return to life in the handsomely illustrated pages of this book.

A fellow of the Royal Astronomical Society, member of the Royal Archeological Society, member of the British Astronomical Association, and a full-time lecturer and author of over twenty books, Dr. Ronan explains each discovery in full. He also gives an account of its later rediscovery and subsequent effect and importance. Artwork specially commissioned for this book helps the reader to understand these early, complex inventions, what they looked like and how they worked.

Highlights of *Lost Discoveries* include the calendar—more accurate than ours—used by the Mayan Indians of Central America, and the atomic theory developed by the Greeks over 2,000 years before the birth of John Dalton. Dr. Ronan explains how, 300 years before Christ, astronomers in Mesopotamia were predicting the movements of the planets and producing astronomical almanacs. He shows how the Ancient Romans built water-powered mass-production factories, and used "taxis" complete with automatic meters.

As Dr. Ronan notes, human ingenuity frequently hit on ideas that are erroneously considered purely modern. *Lost Discoveries* is a fascinating account of these ideas.

BOARD OF MANAGERS MEETING NOTES

Oct. 31, 1973

The 623rd meeting of the Board of Managers was called to order at 8:00 p.m. by President Sherlin in the conference room in the Lee Building at FASEB.

Secretary.—Moved by Dr. Bennett and seconded by Dr. Robbins that the minutes of May 17, 1973 be approved. Passed.

Treasurer.—Budget will be studied at the next meeting.

Nominating Committee.—Dr. Menzer made the following report for the nominating committee: The Committee consisted of Richard K. Cook, Chairman; Jean K. Boek, John A. Eisele, Carl H. Gaum, Peter Heinze, Robert E. Menzer. The committee met on Oct. 16 and offers the following slate for the upcoming election: For President-elect: George Abraham, Raymond J. Seeger; Secretary, Patricia Sarvella, Mary Aldridge; Treasurer, Nelson Rupp, Alred Weissler; Managers-at-large, William Bickley, John Honig, Howard Laster, Richard Farrow. All of the persons nominated have agreed to run and serve if elected. Moved by Dr. DePue and seconded by Dr. Bennett that the report of the nominating committee be accepted.

Meetings.—A list of the programs for 1973–74 was passed out by Dr. Abraham. A commendation was given to him for having a complete program at this time of year.

Grants-in-Aid.—Dr. Shropshire reported that letters have been mailed to the school liaison people. Two student reports were passed around.

Membership.—It was moved, seconded and passed that the following nominees be elected to fellowship: Barney J. Conrath, John O. Corliss, J. Robert Dorfman, Virgil C. Kunde, John D. Mangus, John C. Pearl, James J. Rhyne, Edward J. Finn, Carla G. Messina, Harold J. Raveche, Fred Schulman. The following new delegates were also made fellows: W. T. Bakker, Harry Fine, Michael Chi.

JBSEE.—Dr. Sarvella reported that the Blue Books would be out in about 2 weeks. Dr. Peter Heinze, 11411 Cedar Lane, Beltsville, Md. 20705 will edit the calendar of events for *The Reporter*. Please mail any upcoming scientific events to him.

Scientific Achievement.—Nominees for awards for scientific achievement are to be sent to Dr. Aldridge.

Teller.—Mr. Detwiler is reported on the mend.

Membership Promotion.–Dr. O'Hern has accepted this position and is soliciting applications.

Announcements.—Dr. Weissler attended the ceremony presenting the book *Chemistry in Action*.

A Symposium with 8 co-sponsors on "Statistics and the Environment" is scheduled for March 6, 7 & 8. WAS will publish the proceedings in the Journal. The Federal Highway Administration

has given us a grant for $2500 and an application has been submitted to EPA for the same amount to help defray publication costs.

New Business.—Dr. Gaum questioned whether WAS has any plans to celebrate the bicentennial. WAS will investigate how we can become involved.

It was suggested that emeritus and retired members should be identified in the membership lists.

The meeting adjourned at 9:15 p.m.—Patricia Sarvella, *Secretary.*

SCIENTISTS IN THE NEWS

Contributions in this section of your Journal are earnestly solicited. They should be typed double-spaced and sent to the Editor three months preceding the issue for which they are intended.

DEPARTMENT OF AGRICULTURE

Lloyd V. Knutson, Agricultural Research Service, has been named Chairman of the Insect Identification and Beneficial Insect Introduction Institute, Beltsville Agricultural Research Center. Formerly a systematic entomologist in the Institute's Systematic Entomology Laboratory, Dr. Knutson will continue his taxonomic studies on the snail-killing flies and will enlarge his field investigations into the role they play in the control of schistosome-bearing snails. He will also play a leading role in ARS efforts to make the biological control of insect and weed pests more effective.

NATIONAL INSTITUTES OF HEALTH

Mark Winton Woods, research biologist, Cytochemistry Section, Laboratory of Biochemistry, NCI, recently retired after over 25 years of Federal service.

Before joining the Government, Dr. Woods spent 3 years in active duty with the U.S. Navy and a decade of teaching and research at the University of Maryland.

He was one of the first American scientists to stress the role of both mitochondria and viruses in plant and animal heredity, metabolism, and growth and also their great importance for cancer research.

In 1965 he shared the Gerhard Damagk prize for cancer research, showing the specific importance of glucose metabolism in the development and growth of hepatomas, and he continued experimentation in this field until his retirement.

Dr. and Mrs. Woods are now living in Sun City, Ariz.

Koloman Laki, National Institute of Arthritis, Metabolism, and Digestive Diseases, has been given the James F. Mitchell Foundation award for his research on the cardiovascular system.

Dr. Laki, who is chief of the Laboratory of Biophysical Chemistry, was cited for his discovery of Factor XIII, a plasma transglutaminase, which plays an important role in hemostasis and wound healing.

Dr. Laszlo Lorand, department of chemistry, Northwestern University, co-discoverer of Factor XIII, and Dr. A. G. Loewy, department of biology, Haverford College, shared the award with Dr. Laki.

The award ceremony, held at Sibley Memorial Hospital in Washington, D.C., on Nov. 16, included a half-day symposium featuring research presentations by noted investigators in the field.

Dr. Laki has recently returned from Budapest where he participated in ceremonies honoring Albert Szent-Gyorgyi, Nobel Laureate, on his 80th birthday.

Dr. Laki attended the ceremonies at the invitation of the Hungarian Academy of Sciences.

Stephen D. Bruck, the National Heart and Lung Institute, has authored a book,

entitled *Blood Compatible Synthetic Polymers—An Introduction,* published in December by Charles C. Thomas, Springfield, Ill.

Dr. Bruck is program director for Biomaterials in the NHLI Division of Blood Diseases and Resources.

Marshall W. Nirenberg, chief of the Laboratory of Biochemical Genetics, National Heart and Lung Institute, recently received an honorary doctorate in medicine from the University of Pavia in Milan. The award was presented by Prof. Antonio Fornari, rector of the University. Dr. Nirenberg, who shared the 1968 Nobel Prize in Physiology, was cited for "special merits in the biological sciences."

NEW FELLOWS

Stuart A. Aaronson, Head, Molecular Biology Section, Viral Leukemia & Lymphoma Branch, Viral Oncology, NIH, in recognition of his outstanding contributions to understanding the relationship of viruses to cancer. *Sponsors:* Robert M. Stephan, Bernice E. Eddy, Dean Burk.

Frank J. Adrian, Assistant Group Supervisor, Applied Physics Lab., Johns Hopkins Univ., for theoretical and experimental investigation of the structure of molecules and the solid state. *Sponsor:* Kelso B. Morris.

James S. Albus, Section Head, Cybernetics and Subsystem Development Section, Goddard Space Flight Ctr., in recognition of his advances in artificial intelligence; developing a model for memory in the brain. *Sponsors:* P. E. Landis, Helmut Sommer.

Anton M. Allen, Chief, Comparative Pathology Section, NIH, in recognition of his significant contributions to the knowledge of diseases in research animals. *Sponsors:* Robert M. Stephan, Bernice E. Eddy, Dean Burk.

Lowell D. Ballard, Mechanical Engineer, National Bureau of Standards, for contributions to vibration analysis and in particular his original method of absolute measurement of mechanical shock; and his contribution to physics by the introduction of a new method of measuring the acceleration of gravity. *Sponsors:* Grover C. Sherlin, Russell W. Mebs, Nelson Rupp.

Jay P. Boris, Division Consultant, Plasma Physics Div., Naval Res. Lab., for contributing major advances to the state of the art in computational physics. *Sponsor:* Kelso B. Morris.

Joseph M. Botbol, Project Chief of Computer Graphics Development, U.S. Geological Survey, for outstanding achievement in the application of computer technology to geological data. *Sponsor:* Kelso B. Morris.

Alfred F. Campagnone, Mechanical Engineer, U.S. Atomic Energy Commission, in recognition of his contribution to mechanical engineering and in particular for his development of standards for concrete radiation shields and design criteria for mechanical system components. *Sponsors:* Grover C. Sherlin, Howard B. Owen, Russell W. Mebs.

Barney J. Conrath, astrophysicist, NASA Goddard Space Flight Ctr., for deriving the Martian temperature field, water vapor distribution, and topography from infrared spectra of Mariner 9. *Sponsors:* Kelso B. Morris, Mary H. Aldridge.

John O. Corliss (From Member to Fellow), Professor & Chairman, Dept. of Zoology, University of Maryland, in recognition of his contributions to

knowledge of the protozoa with special emphasis on the biosystematics of ciliates in the genus *Tetrahymena* and in recognition of professional leadership in zoology. *Sponsors:* William E. Bickley, Conrad B. Link, Robert E. Menzer.

J. Robert Dorfman, Professor, Institute of Fluid Dynamics & Applied Mathematics, University of Maryland, for basic research in the kinetic theory of gases and in hydrodynamics. *Sponsors:* Kelso B. Morris, Mary H. Aldridge.

Ronald Fayer, Senior Parasitologist, U.S. Dept. of Agriculture, for cultivating *Sarcocystis* in cell culture and establishing its proper taxonomic status. *Sponsors:* Robert M. Stephan, Bernice E. Eddy, Dean Burk.

Earl H. Fife, Jr., Chief, Dept. of Serology, Walter Reed Army Institute of Research, in recognition of his leadership in the development and improvement of serodiagnostic tests for microbial diseases and delineation of auto-antibodies in health and disease. *Sponsors:* Howard E. Noyes, F. Marilyn Bozeman, Ruth G. Wittler.

Edward J. Finn, Assoc, Professor of Physics, Naval Res. Lab., in recognition of his contributions to the teaching of physics both as a lecturer and as an author of a text translated into nine languages. Additional recognition is merited by his long service to the Committee on the Encouragement of Science Talent. *Sponsors:* Grover C. Sherlin, Richard K. Cook, Nelson W. Rupp.

Richard S. Fiske, U.S. Geological Survey, for creative contributions to fundamental understanding of volcanic processes. *Sponsors:* Kelso B. Morris, Mary H. Aldridge.

David R. Flinn, Research Chemist, Naval Research Lab., in recognition of his contribution to electrode kinetics, and in particular his researches on the mechanism of the hydrogen electrode reaction. *Sponsors:* Kurt H. Stern, Fred E. Saalfeld.

Moshe Friedman, Research Physicist, Plasma Physics Division, Naval Res. Lab., for sustained creative experiments with relativistic electron beams. *Sponsor:* Kelso B. Morris.

Rabindra N. Ghose, President and Chairman of Board of Directors, American Nucleonics Corp., Los Angeles, Calif., for contributions in the field of microwave theory and techniques, and novel antenna concepts. *Sponsors:* George Abraham, Leland D. Whitelock.

Charles M. Guttman, Chemist, Dielectric & Thermal Properties Section, NBS, for his experimental work with polymers. *Sponsor:* Kelso B. Morris.

Melvin H. Heiffer, Chief, Dept. of Pharmacology, Walter Reed Army Inst. of Res., in recognition of his contribution to pharmacology as it relates to development and evaluation of experimental drugs for therapy of refractory cases of malaria. *Sponsors:* Howard E. Noyes, Ruth G. Wittler, Ronald E. Ward.

(Mrs.) Hope E. Hopps, Chief, Immunology Section, Lab. of Viral Immunology, NIH, for her contributions to rickettsiology and virology, and in particular her researches on rubella virus and the serological diagnosis of rubella infection. *Sponsors:* Mary Louise Robbins, Rudolph Hugh, Margaret Pittman.

Kun-Yen Huang, Assoc. Professor of Microbiology, George Washington Univ. School of Med., for contributions to microbiology and in particular, research on the method of action of interferon, especially in relation to non-viral organisms. *Sponsors:* Mary Louise Robbins, L.F. Affronti.

William R. Krul, Research Plant Physiologist, USDA, in recognition of his original and creative contributions in plant physiology, especially his work on

the mechanism of action of plant growth substances and their movement in higher plants. *Sponsors:* W. Shropshire, Jr., Robert L. Weintraub.

Virgil C. Kunde, Astrophysicist, NASA, Goddard Space Flight Ctr., for deriving the Martian temperature field, water vapor distribution, and topography from infrared spectra of Mariner 9. *Sponsors:* Kelso B. Morris, Mary H. Aldridge.

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Richard Stevens Burington

Richard Stevens Burington died December 24, 1973, at the age of 72. He retired in 1971 after 30 years of civilian service with the Navy Department. His latest position was in Naval Air Systems Command where he was Chief Mathematician and consultant to the staff on scientific policy matters.

He was born in Columbus, Ohio in 1901. He graduated from Ohio State University in 1925 and received his Ph.D. in mathematics from there in 1931. From 1926 to 1941 he was professor of mathematics at Case-Western Reserve University in Cleveland, Ohio.

Dr. Burington is the author of many research and educational papers. He is senior author of a widely used textbook called *Higher Mathematics*. He is known internationally for his *Handbook of Mathematical Tables and Formulas* which has been printed in five editions. He also co-authored the *Handbook of Probability and Statistics*.

He joined the Navy Bureau of Ordnance in 1941 where he became a pioneer in weapons effectiveness analysis. He first worked on the mine and anti-submarine systems needed in World War II. This grew into a wide range of analyses of naval weapons of all types, including the anti-air systems used against kamikaze attacks, and the beginnings of the nuclear era of weapons. He participated in the first atomic tests in the Pacific. He also was influential in establishing quantitative systems analysis as the Navy moved into the missle age. He was instrumental in planning the initial design and use of modern electronic computer systems.

Dr. Burington had the Distinguished Civilian Service Award and the Meritorious Civilian Service Award from the Navy Department for work done during the War. Among his numerous other awards is the Superior Civilian Service Award presented by the Navy Department upon his retirement. He is a member of the honorary societies Phi Beta Kappa, Sigma Xi and Pi Mu Epsilon.

Throughout his career Dr. Burington has been active in various mathematical, educational and engineering societies. He organized the Mathematical Division of the American Society for Engineering Education and served as its chairman. He has served on the Council of the ASEE. For several years he was a member of the Committee on Evaluation of Engineering Education, whose published recommendations have resulted in major curricula changes in the engineering schools of the United States. He has served as the Chairman of the Applied Mathematics Committee of the American Mathematical Society. He is a member of the Cosmos Club of Washington.

Professional societies of which Dr. Burington was a member include the following: Washington Academy of Sciences; American Mathematical Society; American Association for the Advancement of Science (Fellow); Philosophical Society of Washington; Mathematical Association of America; Institute of Mathematical Statistics; American Physical Society; U. S. Naval Institute; American Statistical Association; American Society for Engineering Education; Operations Research Society (Fellow); Society for Industrial and Applied Mathematics; Association of Naval Weapons Engineers and Scientists.

He is survived by his wife, Jennet, of 1834 N. Hartford St., Arlington, Va., and three children, Artha Jean Snyder of Darnestown, Md., Richard Edward Burington of Alexandria, Va., Juanita Sulmonetti of Baltimore, Md., and six grandchildren. A brother, Arthur M. Burington of Smithville, Ohio, and a sister, Mrs. Florence Bucher of Columbus, Ohio, also survive.

Sebastian Karrer

Mr. Sebastian Karrer of Scientist Cliffs died at Johns Hopkins Hospital, Baltimore, Friday, Dec. 7, 1973, aged 84.

He is survived by his wife, Mrs. Annie May (Hurd) Karrer, Scientist Cliffs; a brother, Lawrence Karrer of Bellevue, Washington; three sisters, Mrs. Clara Young of Seattle, Mrs. Joanne Armstrong of Griffin, Ga., and Mrs. Roselle Hersh of Cleveland Heights, Ohio; and nieces and nephews.

Born April 10, 1889 in Rich Hill, Mo., he grew up in and near Roslyn, Wash. His education included an A.M. degree from the University of Washington (Seattle) in 1913, and a Ph.D. degree in physics in 1918 from the University of Illinois. His scholarship at these institutions was recognized by election to Phi Beta Kappa, Sigma Xi and Gamma Alpha.

In 1919 he came to Washington, D.C., to head the physics division of the Fixed Nitrogen Research Laboratory, which developed domestic production of nitrogen compounds for munitions and fertilizers.

From 1926–46 he was Director of Research for the Baltimore Gas and Electric Co. There he invented the now widely-used thermoelectric device known as BASO, for automatic shutoff of gas when the pilot light fails. For this invention, he received the Modern Pioneer award of the National Association of Manufacturers.

In 1942–45 he was a consultant for the Applied Physics Laboratory of Johns Hopkins University, Silver Spring, where his invention of a rugged vacuum tube made possible the proximity fuse, a major contribution to the World War II effort. For this he received a Naval Ordnance Development award.

He also served as consultant for the National Defense Research Committee.

Following the war in 1946, he became chief consultant for the research and development division of the School of Mines in Albuquerque, N. Mex.

From 1948–1958 he was director of research for the Milwaukee Gas Specialty Co. that was making the gas shutoff device, later called the BASO Co.

In 1958–59 he was consultant for the electrical products division, and in 1960–61 was Associate Director of Central Research for Minnesota Mining and Manufacturing Co. There he and his associates continued work begun at BG&E that led to the gas industry's first thermoelectric generator, precursor of later devices useful in other fields, such as SNAP, basic for space satellites.

During the period from 1958 to 1962 he was also a consultant for the U.S. Naval Weapons Laboratory in Dahlgren, Va. From 1964 to 1968 he was a research associate of the Georgetown University Observatory.

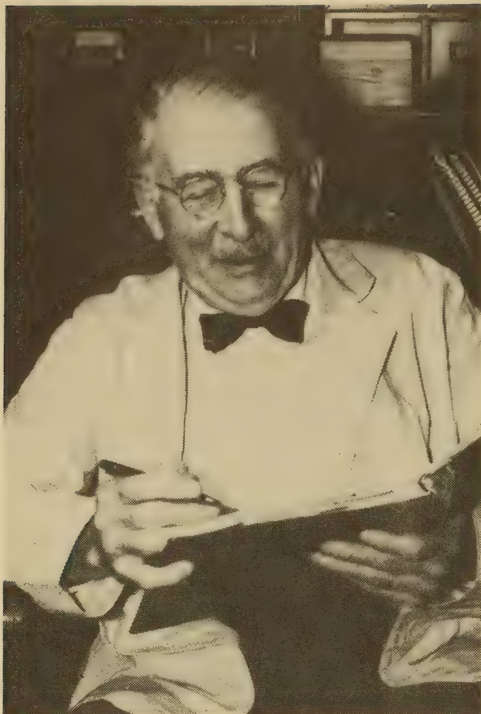
He was a member of the Newcomen Society, the Washington Academy of Sciences, a Fellow of the American Association for the Advancement of Science, and of the American Physical Society, and a director of the Maryland Academy of Sciences. He was a member of the American Chemical Society, the Philosophical Society of Washington, the Crystallographic Institute, and the Geophysical Institute.

Some of his major contributions were in the fields of production, distribution, and use of gas and electricity; refrigeration; thermoelectric control devices; solid state of matter; and thermoelectricity.

Leland W. Parr

Leland W. Parr, of Scientists' Cliffs, Port Republic, died in Baltimore, Maryland, on December 15, at the age of 81. Dr. Parr retired to his home in Scientists' Cliffs in 1960 after a distinguished career as a medical educator and microbiologist at the School of Medicine of George Washington University in Washington.

Leland Wilbur Parr was born in Cooksville, Illinois, on November 2, 1892. He was educated at Drake University and the University of Chicago, receiving the B.S. from Chicago in 1916 and the Ph.D. in 1923. He taught at Assiut College in Egypt from 1916 to 1919, and at the American University of Beirut in Lebanon from 1923 to 1930. He worked as chief bacteriologist at the Rockefeller Foundation Field Research Laboratory in Andalusia, Alabama from



Leland W. Parr

1930 to 1932, and then he joined the faculty of George Washington University in Washington, D.C. From 1938 to 1958 he served as Professor and Chairman of the Department of Bacteriology, Hygiene and Preventive Medicine. Emeritus rank was conferred upon Professor Parr in 1958 by the University.

Professor Parr was a fellow of the American Academy of Microbiology, and a member of the Washington Academy of Medicine, the American Society of Experimental Pathology, the American Public Health Association, the Society of Experimental Biology and Medicine, the American Genetic Association, and the American Association for the Advancement of Science. He served as President of the Washington Academy of Sciences in 1943, and as Secretary-Treasurer of the Society of American Bacteriologists from 1944 to 1949. He helped found the Association of Teachers of Preventive Medicine and was chairman of that group in 1949. He was also an organizer of the American Institute of Biological Sciences. For many years he was an adviser to the Director of Selective Service, and he also served as consultant to the National Institutes of Health, the Surgeon-General of the Army, and the Veterans Administration, for which services he received an Army Service Forces Award. He was a member of Phi Beta Kappa, Sigma Xi, Nu Sigma Nu, Alpha Omega Alpha, Phi Delta Theta, and the Cosmos Club. He was the author or coauthor of many research papers, the "Dr. Leyland" series of articles in *Hygeia* magazine, and the books *Anthropology of the Near East* (1934) and *Laboratory Methods of the United States Army* (1944).

In 1939 Professor Parr and his wife built the home in Calvert County to which they retired in 1960. Dr. Parr was an active member of Christ Church, and on the County Commission on Aging, which was instrumental in bringing the Calvert Nursing Home to the County.

He is survived by his wife, the former Grace Ghormley; his daughter Mrs. Patricia Bash of Scientists' Cliffs; his son Robert of Baltimore; his brother Arthur of Newman, Illinois; his sister Mrs. Edith Santis, also of Newman; six grandchildren and three great grandchildren.

Contributions in Professor Parr's memory may be sent to The George Washington University School of Medicine, 2300 Eye Street, N.W., Washington, D.C. 20037.

JOURNAL OF THE WASHINGTON ACADEMY OF SCIENCES

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Symposium Issue

CONTENTS

(Continued on Back Cover)

The Journal

This journal, the official organ of the Washington Academy of Sciences, publishes historical articles, critical reviews, and scholarly scientific articles; proceedings of meetings of the Academy and its Board of Managers; and other items of interest to Academy members. The *Journal* appears four times a year (March, June, September, and December) — the September issue contains a directory of the Academy membership.

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STATISTICS AND THE ENVIRONMENT

A Forum For Interdisciplinary Interaction

Hugh Dryden Memorial Auditorium, National Academy of Sciences, Washington, D.C.

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Steering Committee Message

Our objective is "to provide a forum for the interchange of ideas of mutual interest among experts in toxicology and environmental areas with specialists in the statistical techniques of data gathering and analysis."

This is not a meeting where statisticians will speak statistically to their colleagues, or environmentalists conversing in their own language to their co-scientists.

It is hoped that attempts to solve environmental problems will be enhanced by an interdisciplinary approach resulting from the communication among the pertinent professions.

Beatrice S. Orleans
General Chairperson

Acknowledgments

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For recording the Symposium we acknowledge the assistance of David S. Garber, a specialist in corporate life development recording. As an industrial personnel specialist he has had wide experience recording and communicating every phase of corporate life from human factor changes in top management personnel to producing, indexing and editing minute-by-minute archival recordings of important meetings and symposia.

For transcribing and typing of discussion sessions and manuscripts we acknowledge the assistance of Suzanne C. Ressler.

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Introduction to the Symposium

Ralph C. Wands

Director, Advisory Center on Toxicology, National Academy of Sciences, 2101 Constitution Ave., N.W., Washington, D. C. 20418

It is my pleasure this morning to introduce several people to you and to make a few general announcements before we get the program underway. The person I would like to introduce to you in particular is perhaps the spark plug of this whole operation. The Symposium began, as far as I was involved at any rate, in a series of conversations between people from the Naval Ship Systems Command, particularly Dr. Huth and Bea Orleans in conjunction with Dr. Seymour Friess at the Naval Medical Research Institute and myself. Out of those several conversations of the four of us this Symposium continued to grow. Today we are seeing the culmination of those early brainstorming sessions. As I have said, the spark plug is the one I want to introduce to you—Bea Orleans, who is the chairperson of this Symposium. Bea has steadfastly refused to say anything more than "thank you." It is we who say "thank you" to her.

At this point I would like to acknowledge the support of our co-sponsors—for moral support and, in some instances, financial support. They are listed on the cover of your program. Let me review the names of the organizations that have been behind the planning and operation and conduct of this Symposium: The American Society for Quality Control; the Washington, D. C. Section, American Statistical Association; Committee on National Statistics-National Academy of Sciences-National Research Council; Committee on Toxicology of the NAS-NRC; Environmental Protection Agency; Federal Highway Administration; National Bureau of Standards; National Institute for Occupational Safety and Health; National Institute of Environmental Health Sciences; Society of Toxicology; Washington Academy of Sciences; Washington Operations Research Council; and Washington Statistical Society. Of these we are particularly indebted to the Federal Highway Administration, the EPA, and NIOSH for providing financial support for the conduct of this Symposium. We are also indebted to the Washington Academy of Sciences for publishing the proceedings. Our Steering Committee is also in your program. I would like to pay a special word of thanks to those members of the Program Committee—Dr. John Mandel, Dr. Seymour Friess, and Dr. Fred Leone—for putting together this outstanding group of speakers and setting the stage that would attract all of you from the far regions of our country. Many of you come from as far as the West Coast to be with us. We do appreciate your interest and we look forward to having your vital and active participation in the discussions. We are naturally very grateful to all our various speakers for their willingness to share their time and thoughts with us in this Symposium.

Before I introduce our speakers, there are a few general announcements which should be made. Tomorrow afternoon we will be passing out evaluation sheets. We would like to know your impressions, your opinions, and your comments on the Symposium. These will be very valuable to us in

determining the possibility of planning additional Symposia of this sort or on additional topics.

I have two changes to announce in our program. One of our panelists, Dr. Boyd Shaffer, is unable to be with us this afternoon. We are fortunate to have as his replacement Dr. Harold Peck, who is an M.D., a toxicologist and member of the Committee on Toxicology. He is in charge of toxicology and safety evaluations for Merck and Company. One other change: Our chairman for tomorrow morning's session on air pollution will be Mr. Henry Lathrup. Also, Dr. David Solomon, who was scheduled to chair tomorrow morning's session, is not able to be with us—he is asking his Deputy, Mr. Henry Lathrup, to fill in for him. Mr. Lathrup is the Deputy Chief of the Environmental Design and Control Division of the Federal Highway Administration.

I would like to move now to the introduction of our program for this morning. Some time ago when some of us on the Academy staff were discussing this Symposium—Statistics and the Environment—one of them said, "Well, a symposium had been defined as the last resort of the intellectually bankrupt." As I though about that a little bit I decided this is true in some regards for this Symposium today. As bankrupt people are pretty much faced with insurmountable problems, certainly those of us in this Symposium are faced with some insurmountable problems, particularly as we may well not be intellectually bankrupt in our own area of specialization but where we are totally ignorant or at least only moderately aware and competent in the other person's area of specialties. And as we try to improve and correct the damage that has been done to our environment we find that it requires many, many disciplines—many, many areas of specialization. Two of the critical ones are those concerned with the health aspects of our environment and those concerned with statistics as their field of specialization. It has been our experience, as members of one of the health science professions, that very few of us have any more than a nodding acquaintance with statistics or even with statisticians, and when we need their help we desperately need it. Certainly this is true for the problems which we will be discussed at this meeting. What I am saying is that it is most appropriate in terms of this concept of intellectual bankruptcy in the other person's area of specialty for these two groups of professionals—the statisticians and the health scientists—to get together to talk about the problems relating to our environment. This is the total purpose of our Symposium today—to bring together on a face-to-face, one-to-one basis representatives of both disciplines so that they can begin not only to get acquainted personally but to share ideas and concepts in the hopes of developing some new approaches to some of the very critical problems that will be discussed here. In this regard the Program Committee has limited the speakers and the topics to statistics as they may be applied to problems involving the toxicity of chemicals entering our environment. We recognize that the concepts and principles that may be developed here are quite likely to be applicable to other problems of the environment involving low levels of physical insults such as noise, trauma, and radiation. Some of you in this audience are knowledgeable in those specific fields. We hope that your experience will be shared with the rest of us as we tackle these problems of chemical toxicity. Particularly, I am thinking of the experience of the folks in the field of nuclear radiation where they have been struggling with the issue of identifying effects at a very low level of response from extremely low levels of exposure. This is the kind of problem we are running into as chemicals enter into our environment. We do hope that the people with those kinds of backgrounds will share them with us in the discussion.

Our first speaker of the morning is

a gentleman with whom I have been acquainted for the last several years and have thoroughly enjoyed my relationship with him—he is a delightful person and an extremely skilled professional. He holds an M.D. degree from the University of Pittsburgh in Epidemiology and Internal Medicine. He taught at Western Reserve University in Cleveland and in the late 60's he moved into the Federal establishment in the field of air pollution control. Today he is special assistant to the Administrator of the Environmental Protection Agency with responsibilities for health effects. It is a pleasure to introduce to you Dr. Vaun Newill.

Regulatory Decision Making: The Scientist's Role

Vaun A. Newill, B.S., M.D., S.M. Hyg.

Office of the Administrator, Environmental Protection Agency, Washington, D. C.

Regulatory decision making implies that there is a problem that needs regulation and that some organization or group has the responsibility, interest, authority and means to do something about it. Having recognized at different points in time that certain environmental exposures were a threat to the health of our population, the Legislative and Executive Branches of our federal government developed laws and the institutional arrangements to deal with such problems. The early efforts brought environmental sanitation, which, when coupled with other medical advances, particularly the advances in the treatment of acute infectious diseases, contributed to a longer expectation of life. This increase in the average number of years that the population is living permits more persons to develop the chronic degenerative diseases and other manifestations of longer exposure to environmental contaminant risks. The major task of the environmental regulatory agencies is to reduce such environmental risks and their resulting disbenefits.

For discussion the environmental regulatory process will be presented in simplistic terms around 4 major steps (Fig. 1) (Newill, 1972a), namely: (1) Problem identification and assessment, (2) alternative control strategies, (3) implementation and enforcement of an action program, and (4) action program assessment.

Environmental Problem Identification and Assessment

Environmental problems are identified in two general ways. One is by focusing on the environmental factors (agents) to which the population is exposed. Another is by focusing on disease determinants and mechanisms. The two approaches can be easily visualized by considering a 4-fold table (Table 1) that relates environmental contaminant exposure to disease. It is natural that the regulatory agencies primarily approach these problems by investigations that relate the effects that result from exposure to the environmental contaminant while the health agencies, at least classically, approach the problems through studies related to the disease process

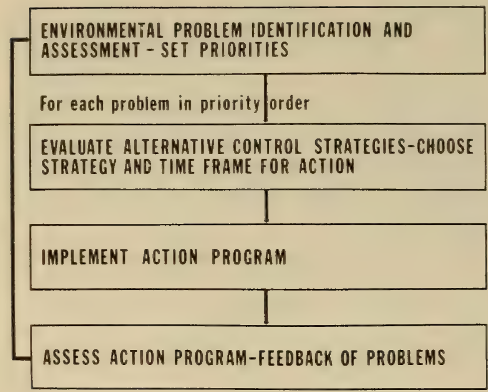

Fig. 1. Simplified schematic of the environmental control process.

itself. When the resulting data from either approach demonstrates or even strongly suggests a causal relationship, the suspect agent becomes a candidate and strong contender for regulatory action.

The number of potential environmental contaminant problems is quite large and both the human and financial resources to study them are limited. Thus, it becomes essential to deal with such problems in a priority order. Factors considered in placing the problems in a priority order include such considerations as: (1) characteristics of the pollutant, i.e., ubiquity, expected levels of exposure to the population, innate toxicity, bioaccumulation and persistence; and (2) the pollutant's potential for playing an important role in public health problems, i.e., affecting the frequency, severity or trend pattern of a specific disease. Pollutants suspected as being related to diseases that are common, severe, and increasing in prevalence should gain the highest priority for regulatory action (Newill, 1972b).

Table 1.—Relationship between environmental factor and disease

Environmental Factor	Disease	
	Yes	No
Yes		
No		

To the best of my knowledge no major systematic effort aimed at establishing such a set of priorities for individual problems is underway, although there have been several efforts that have attempted this process on a much broader problem scale (Flinn and Reimers, 1974). Before ending this cursory discussion of the problem of prioritization, it should be mentioned that public interest in a specific problem can increase the priority beyond that which would be assigned without such public interest. The number of such crisis reactions could be usefully reduced and more effort should be expended to anticipate and defuse them.

The identification process needs to be followed by an in-depth assessment of each problem in priority order to decide if, on the basis of existing information, (1) no regulatory action is required, (2) adequate information is available to proceed with preparation of a set of control options, or (3) insufficient information is available and additional research is required. Parenthetically, it should be noted that much more knowledge is required to decide how to deal with a problem than is necessary merely to identify the problem.

The number of problems referred for research is large, certainly more than available resources can accommodate simultaneously. Thus, there must be a second order prioritization, where each new research problem must be evaluated to learn how well it can compete for resources against the total set of problems already under investigation.

My remarks will not address the specific research that needs to be performed but will rather address issues that affect the research. One such issue is, "Should the research effort related to a specific problem be supported by the public or the private sector?" As most of you know, the responsibility for the research on the effects of general ubiquitous air and water pollution problems have been accepted by the public sector whereas the private sector is expected to provide the effects research information for the registration of a

pesticide, or acceptance of a new drug. In many areas, however, no such clear responsibility can be assigned for the research activity. Furthermore, the quantity of research for which the public sector is responsible is so large that only a limited amount can be performed; thus in order to assure that the more needed research is done, acceptable ways must be found for the public and private sectors to cooperate to speed the process along.

As indicated earlier more information is necessary to know what regulatory action to take than is necessary to identify a problem. When regulatory action is rushed prior to the development of sufficient information for regulatory decision making, this rush assures that the control action will be prudent, supposedly conservative and in the best interest of the population. Prudent actions, however, tend on the average to be more stringent than necessary, thus more expensive, and not necessarily in the best interest of the population they are to protect.

Now let us consider some issues with which the environmental health scientist must deal in the detailed assessment of specific problems.

Integrated information from several disciplines is useful in the health assessment of these problems. The major disciplines involved are epidemiology, clinical research and the whole set of experimental studies that I will include under the term toxicology, being fully aware that many other disciplines will be involved in these studies (Fig. 2).

Each of these discipline approaches has useful and, in some instances, unique information to provide. A well-designed program, integrating the unique capabilities and crosswalks between these disciplines can provide a most satisfactory basis for regulatory action. Epidemiology can provide studies of population exposure in real life settings. The advantages of epidemiology are the natural exposure, no need for extrapolation of data to the human, the most vulnerable groups in the population can be studied, and both current and long-term low-level exposures can be evaluated. The major problems relate to

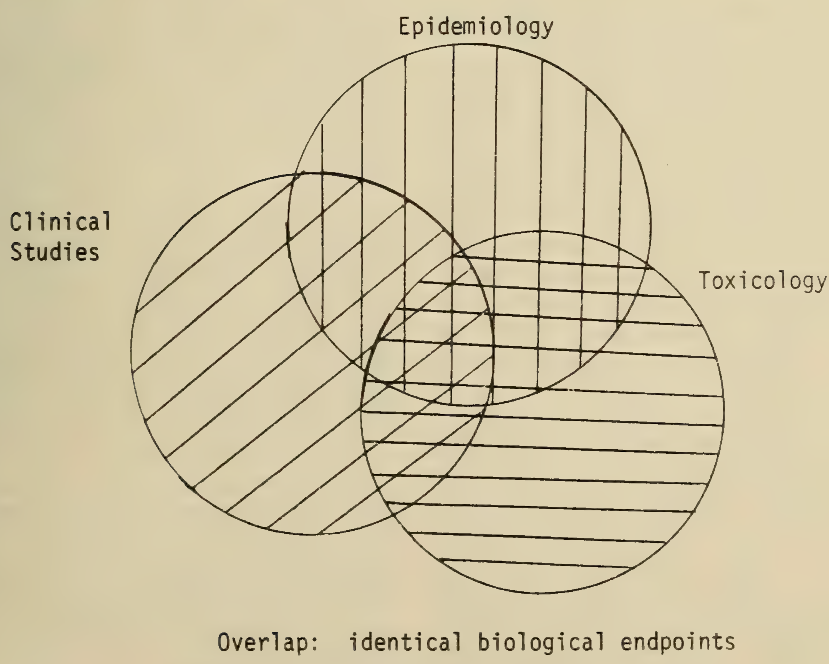

Fig. 2. Methods to demonstrate biological response to pollutants.

quantifying the exposure, to dealing with the many co-variates, to obtaining dose-response data and to deciding association vs. causation.

Clinical research studies can be used to gather human data on either normal or diseased persons regarding absorption, metabolism and excretion of pollutants and can be used for in-depth studies of humans accidentally exposed to high levels of pollutants where new study parameters and response indicators can be identified. The advantages of clinical studies are: the pollutant exposure is controlled so that improved dose measurements are obtained; since each person can usually be used as his own control, covariates are well controlled; vulnerable subjects can be included in the studies; cause-effect relationships are more easily ascertained; there is no need for extrapolation to humans; and thus the derived information gives maximum input into standards. The problems are that the exposure is artificial, there can be no long-term exposures thus only acute effects are determined, and there can be real hazard to the exposed person.

Toxicologic studies can use many response systems, such as whole animal, organs, cells or biochemical systems. The advantages of toxicologic studies are: maximum dose-response data can be obtained, though this is incomplete at the low end of the curve; data acquisition is rapid; cause-effect relationships are more sure; and mechanism of response studies, such as kinetics of pollutant absorption, distribution, metabolism and excretion, can be performed. While known quantitative exposure requirements are most easily satisfied under controlled or experimental exposure settings, appropriate human disease models in animals have not been available and thus have not been evaluated in this manner. Neither can such experimental studies readily identify delayed responses or chronic cumulative effects of exposure. Laboratory experimental studies cannot provide complete assurance of dose effects because community exposures cannot be duplicated in the laboratory. Furthermore, the difficulties of extrapolating the data to the human remains, particularly estimating the threshold of human response.

Planning for the detailed assessment of a pollutant problem or evaluation of the existing information for a regulatory decision can usefully be performed by keeping these three approaches in mind.

Pollutants Response and Human Exposure

"The effects of pollutants on human health depends on the physical and chemical properties of the pollutant, on the duration, concentration and route of exposure and on the human uptake and metabolism of the pollutant. Man's biological response is likewise a function of occupational, psychosocial and climatologic factors and is tempered by the phenomena of tolerance and adaptation. These exposure factors underlie attempts to understand the impact of pollutant exposure on human health."[1]

"The physical and chemical properties of pollutants determine their potential as a health hazard. These properties—including size, density, viscosity, shape, electrical charge, volitility, solubility and chemical reactivity—all affect the absorption, retention and toxicity of the pollutants. Many pollutants do not retain their exact identities after entering the environment. Thermal, chemical and photochemical reactions occur when pollutants move through the environment from source to receptor. These factors affect the final physical and chemical state at the point of human exposure and help determine the toxic potential of the pollutants."[1]

[1] Message from the President of the United States Transmitting the Report of the Department of Health, Education and Welfare and the Environmental Protection Agency on the Health Effects of Environmental Pollution, Pursuant to Title V of Public Law 91-515. 92d Congress, House of Representatives Document No. 92-241, February 1, 1972.

The Response Spectrum[1]

Environmental pollutants can affect the health of individuals or communities over a broad range of biological responses. One can conveniently think of 5 biological response stages of increasing severity as illustrated in Fig. 3: (1) a tissue pollutant burden not associated with other biological changes, (2) physiologic or metabolic changes of uncertain significance, (3) physiologic or metabolic changes that are clear-cut disease sentinels, (4) morbidity or disease, and (5) mortality or death. Boundaries between categories may occasionally overlap. Furthermore, each category shows a range of responses rather than a simple all-or-none phenomenon.

At any point in time more severe effects, such as death or chronic disease, will be manifest in relatively small proportions of the population. In very few cases can death or disease be attributed directly and solely to pollutant exposure. Death and disease are end products of repeated cumulative insults (cumulative risks) from sources such as diet, cigarette smoking, physical inactivity, infectious challenges, and accidental injury. In general, the role of environmental contaminants in the mortality or morbidity experience of a community is difficult to quantify because so many other determinants of death and disease cannot be adequately measured.

The lower levels of the response spectrum shown in Fig. 3 are subclinical manifestations of pollutant exposures. Larger portions of the population are affected at these levels. Pathophysiologic responses such as impaired mucociliary clearance and bronchoconstriction, and physiologic changes of uncertain significance, such as neurobehavioral re-

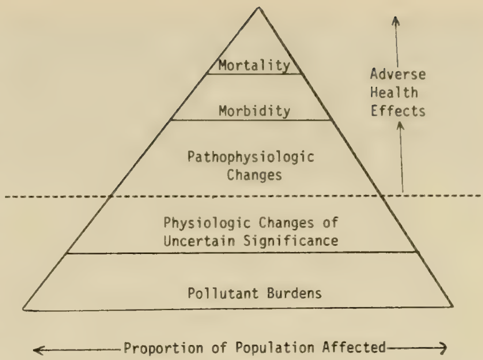

Fig. 3. Spectrum of biological response to pollutant exposure.

sponses, are more adaptable to experimental studies on animals or humans than is the case with acute or chronic disease and can be more readily associated with specific pollutant exposures. Pollutant burdens are tissue residues resulting from pollutant exposure. Pollutant burdens are highly specific effects of exposure, can be readily quantified in population studies and may be used as indicators of environmental quality. If the bridge between the lower and higher levels of the response spectrum can be established, the disease risk associated with pollutant burdens or physiologic changes can be shown, and ultimately the role of pollutant exposure in the total community morbidity and mortality experience can be defined.

Some groups within the population may be especially susceptible to environmental factors. Notably these include the very young, the very old, and those affected by a disease. Thus susceptibility may be temporary or permanent. Inherited abnormalities such as alpha antitrypsin deficiency and abnormal hemoglobins are examples of permanently altered sensitivity. Temporary increased sensitivity may be associated with periods of growth, with weight reduction, with pregnancy and with reversible illnesses.[1]

Diseases commonly result from complex causal webs rather than single factors (MacMahon, et al., 1960). Environmental pollution may contribute a

[1] Message from the President of the United States Transmitting the Report of the Department of Health, Education and Welfare and the Environmental Protection Agency on the Health Effects of Environmental Pollution, Pursuant to Title V of Public Law 91-515. 92d Congress, House of Representatives Document No. 92-241, February 1, 1972.

number of strands to such webs. Other strands may arise from such diverse origins as genetic heritage, nutritional status and personal habits. Moreover, pollutant exposure may alter the severity of disease without altering its frequency.[1]

Exposure Response Matrix

The duration and concentration of pollutant exposure are measures of the total dose to the human. It must be remembered that the rate at which the total dose is received may influence response.

Health effects of an environmental pollutant may be either short-lived (acute) or relatively permanent and irreversible (chronic). Acute or chronic effects may occur after a single exposure to a hazardous substance. This can be illustrated by acute radiation exposure which can cause acute radiation gastroenteritis and chronic leukemia. An air pollution episode may have similar effects though the permanent sequelae of acute episodes have never been adequately studied. Likewise, acute and chronic effects can result from long-term exposure. For example, excess acute respiratory illness and chronic respiratory disease have been repeatedly demonstrated in high exposure cities. Acute effects and short-term exposures are less difficult to study than chronic effects or long-term exposure. Moreover, the effects of dose rate, i.e., large dose in a short interval vs. repeated small doses over a long period, have seldom been investigated systematically. Little effort has been expended on the monitoring of long-term exposure and disentangling the causal webs underlying chronic effects of pollutant exposure. Such effort has been hampered, however, by methodologic difficulties.

[1] Message from the President of the United States Transmitting the Report of the Department of Health, Education and Welfare and the Environmental Protection Agency on the Health Effects of Environmental Pollution, Pursuant to Title V of Public Law 91-515. 92d Congress, House of Representatives Document No. 92-241, February 1, 1972.

Estimation of Exposure

Population exposure to individual pollutants or pollutant mixes (Shy, 1973) changes rapidly with time; they vary with season, day and hour. Also, within short-time frames, our very mobile population moves from indoor to outdoor environments and from one neighborhood to another. Approximately 20% of the U.S.A. population changes residence each year. Many cross-sectional studies of communities have shown that one-third of the population have resided at their current address for only two to three years.

Attempts to derive estimates of long-term integrated exposure, even of small populations in a single neighborhood, are fraught with difficulties in quantifying personal exposure. Quiet, tolerable and small-scale instruments for personal monitoring are being developed. However, when developed they will not reduce the need for stationary monitors even for community research. Control programs will still need to be managed by stationary monitoring systems and integrated exposures, for personal monitors will need to be related to the measurements at stationary sites.

Stationary monitors have inherent draw-backs due to variation in performance over time and between instruments, interferences caused by variations in temperature or in concentration of pollutants, and simple instrument malfunction. Continuous monitoring, required to assess the effect of short-term exposures, is very costly and technologically complex. Furthermore, monitoring equipment is too often dissociated in time and space from measured health effects, especially where chronic effects are considered.

Studies of disease frequency in a large city or metropolitan area often rely on exposure estimates based on one or a few stationary monitoring units. These stations are usually not representative of community-wide exposure and often provide erroneous estimates of exposure in residential areas. Conclusions based

on such results may imply higher exposures than actually occurred in residential areas having pollutant-associated disease excess. On the other hand, ascribing excess chronic disease to pollutant concentrations currently measured may imply lower than actual exposure since, for example, air pollutant levels in larger cities in U. S. A. in the 1940's and 1950's tended to be considerably higher than in 1970 or 1971, when sulfur restrictions and particulate control measures were more prevalent.

Information is seldom available to cope adequately with these methodologic problems. In establishing exposure-response relationships, the weakest links in the association are quantitative estimates of exposure. However, sound criteria require as precise data about exposures as about effects. Actions based on poorly-defined criteria may be either over-restrictive or under-protective of health.

Evaluating Exposure-Response Relationships (Shy, 1973)

Given adequate characterization of exposures and health effects, many additional considerations bear on the scientific validity of the exposure-response relationship. Hill (1965) has given an exposition of criteria that can be used to judge whether an observed exposure-disease relationship is causal. Hill's criteria were developed as guides for occupational health studies. With minimal modification, however, they can be applied to general population studies. These criteria are: Consistency of observed associations, coherence of results, plausibility of the association, and strength of association. Brief descriptions of exposure response gradient, intervention, and control of covariates are included.

1. *Consistency of observed association.*—Consistency of observed association is, perhaps, the most important criterion. Does the health effect occur in various age, sex and race groups? Has the effect been repeatedly observed in different places, circumstances and times? Even small differences that are not quite statistically significant bear great weight in standard setting when the criterion of consistency is met. When the same effect is observed in a variety of population groups under varying conditions and at different times, the likelihood of a constant error or fallacy becomes progressively less.

2. *Coherence of results.*—When animal studies, experimental human exposures, and epidemiologic data are coherent, i.e., they all demonstrate the same or similar health effects of exposure, the bits and pieces of evidence, when brought together, form a mosaic of health intelligence. One study supports another; experimental exposures identify pathophysiologic pathways by which effects observed in epidemiologic surveys become biologically explainable. The three disciplinary approaches discussed earlier are needed to form this interlocking mosaic: animal experimentation, controlled human exposures and epidemiologic studies. Crosswalks between each discipline can be readily identified. Through animal studies, metabolic pathways, organs of response, and effects newly identified in humans can be verified across a broad range of pollutant exposures. In well controlled human exposure studies (as employed extensively in tests of new drugs) results from animal studies can be made more relevant to human exposure at ambient levels. Also, effects observed in population studies can be studied under well controlled human exposures from near zero to ambient levels at varying exposure times. Such results have high immediate utility for short-term air quality standards. Finally, epidemiologic surveys of communities before and after introduction of environmental quality control measures provide unique data for evaluating the adequacy of established standards and demonstrating the health benefits of control. Results from epidemiologic studies provide the impetus to develop animal

disease models from which more complete dose-response curves may be developed.

By reinforcing results obtained in one approach with studies in another, a coherent health intelligence system will provide scientifically strong and readily acceptable guides for costly air quality controls.

3. *Plausibility of the Association.*— Initial studies may uncover unexpected relationships between exposures and effects. Mere statistical associations must be rejected when no reasonable biological explanation, based on experimental evidence, can justify the association. On the other hand, when experimentation points to a disturbed physiologic process which may lead to some clinical manifestation, subsequent epidemiologic studies designed to test such hypotheses are well grounded in biologic plausibility. New exposure-response associations require support from other disciplines before causal inferences can be readily defended.

4. *Strength of the Association.*— When disease frequency is nine to tenfold greater in exposed than in non-exposed populations, the exposure-effect relationship is extremely strong. Relationships of this magnitude have been found in studies of cigarette smoking and respiratory disease including lung cancer and bronchitis. However, pollutant exposures are generally less intense and less reactive than cigarette smoke inhaled deeply into the lungs. Differences in exposure between high and low pollution neighborhoods seldom exceed 2- or 3-fold concentration gradients. At present ambient air concentrations, relative differences in effects between high and low exposure areas are unlikely to be as striking as ratios observed in smokers vs. nonsmokers. However, for common and frequent disease events such as acute respiratory disease, relatively small differences in disease experience can have a large and costly impact.

5. *Exposure-Response Gradients.*— When a stepwise increase in exposure can be associated with a stepwise increase in the frequency of the adverse health effect, the evidence is strong for a cause-effect relationship. Linear relationships over an exposure gradient become increasingly difficult to explain by third intervening variables. Response gradients can be investigated in relation to exposure gradients across geographic areas, differences in length of residence in high exposure areas, and migration gradients constructed from various combinations of childhood and adult exposures of the same individuals. Human exposures occur naturally over an exposure gradient, especially over time and across areas. Exposure-response gradients obviously can be easily created in experimental settings.

6. *Intervention.*—Protection of health requires society to intervene in public exposures to air pollution. This intervention is largely based on observed exposure-health effects associations. As desirable air quality is achieved, will the frequency of adverse health effects be affected? If so, the causal nature of the exposure-response association is strongly supported. The high cost of air pollution control warrants a national program of community health and environmental surveillance in those places where achievement of required air quality will require vigorous abatement efforts.

7. *Control of Covariates.*—The causal nature of an exposure-response association is convincingly identified when, after the effects of known covariates are first displayed, increased disease risk within covariate classes can be clearly demonstrated in high exposure populations. For example, in any studies of chronic bronchitis prevalence, smokers and males show more disease than nonsmokers and females respectively. An air pollution-chronic bronchitis study should reveal the above smoking-sex differences in prevalence rates, thereby assuring readers that the study has internal consistency. If smok-

ing-sex specific groups in high exposure neighborhoods have excess chronic bronchitis, the hypotheses that air pollution exposure causes excess chronic bronchitis is considerably more convincing than if the smoking-sex covariates were not analyzed. Most epidemiologic studies of air pollution require similar analysis of excess disease risk within covariate categories, with particular attention given to age, sex, smoking, socioeconomic level and duration of residence at current location. When covariates are systematically analyzed for relationships to the health indicator under study, residual excesses in disease frequency can be attributed to area differences in pollution exposure with a reasonable degree of confidence.

Once the information regarding a specific problem indicates it is of sufficient risk to require regulatory action then the next step in the control process becomes the focus.

Alternate Control Strategies

Fig. 4 is a schematic diagram presenting several of the principal factors in the decision-making process concerning air pollution control (WHO, 1972). It indicates that the degree of health protection attained is a function of pollution control costs. The minimum acceptable level of health protection is, at the very least, that level necessary to protect from death; and the immediate regulatory action program adopted for environmental pollution control should certainly protect from illness as well.

The degree of health protection to be selected above the minimum acceptable level is a matter for political decision. The appropriate authority must decide on the level of health protection desirable for his society. Increments of health protection above the minimum acceptable level are generally purchased at ever increasing increments in control costs. Furthermore, the cost of the control program is directly related to the deadlines by which it is to be operational; for example, it is more expensive to achieve goals in three years than in 10 years. The zone in which increased health protection (benefit) is obtained at increasing control costs (the cross-hatched area in Fig. 4) is also the region

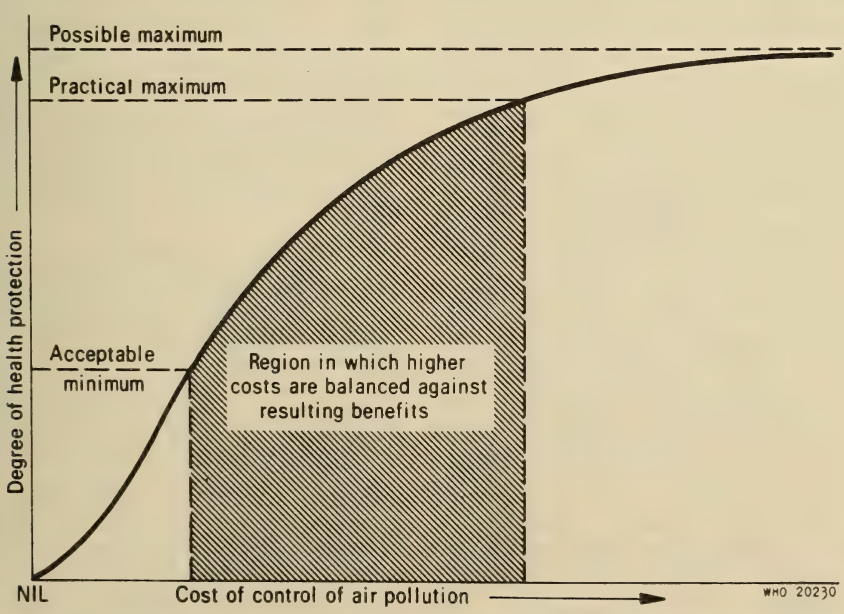

Fig. 4. Schematic representation of degree of health protection as a function of cost of air pollution control.

of social decision-making. The level of health protection desired must, of course, take into account the existing environmental pollution effects, but other considerations are also important, including general social, cultural and economic factors, as well as the magnitude of other health problems. In deciding where to spend limited money resources, the total set of risks that man must bear needs to be considered and the greatest efforts should be expended to reduce the largest risks.

For our U. S. A. society air pollution risks, for example, have been adjudged to be sufficiently significant to justify a major control program. Thus, we have a Clean Air Act, the legislative mandate, and the Environmental Protection Agency, an institutional arrangement, to implement an air pollution control program.

Each regulatory action is taken for society's benefit. However, each action also has a societal disbenefit because it will consume a certain quantity of the society's resources for its accomplishment. Obviously there must be some give and take between the societal benefits and disbenefits. Decisions involving this process are difficult because the group to benefit may be different from the group that will receive the disbenefits, and thus it is difficult to make this an equitable process.

Another important concept follows from further consideration of Fig. 4. Infinite health protection is unattainable; thus society must either decide what level of health risk is acceptable, a decision that will be reflected in the societal investment in environmental pollution control; or the decision will be made indirectly since the societal investment in environmental pollution control will dictate the level of health protection to be achieved.

It is our purpose here to examine the process of making decisions concerning the acceptable health risk, particularly the scientist's role in this process. The scientist's role simply stated is to unravel and develop understanding of the problem. The scientist should be able to provide judgments that will bound the problem and make suggestions for action that will aide the administrator/politician in making appropriate decisions.

The need for regulatory action is predicated on the demonstration of an effect at the levels of a pollutant to which the population is or can be predicted to be exposed. The kind of control options available to prevent such effects and to choose among are environmental standard setting, registration, certification, licensing, limiting use, banning and economic incentives such as effluent charges or taxes. For simplicity, the only option that will be addressed is environmental standard setting.

An environmental standard in itself is a complex issue. The ambient standard is the maximum permissible level of exposure of the public to a pollutant permitted for a specified period of time. For a pollutant that is only present in the air, the environmental standard is the primary ambient air quality standard. However, when the pollutant is one where human exposure comes not only from the air, but from the food and water as well, the environmental standard becomes a set of standards, each representing an allocation of a portion of the permissible exposure through each of the media from which exposure comes. No such environmental standards have been set in the U. S. A. to date (Newill, 1973).

The actual permissible level of exposure permitted must be decided in some philosophical framework. At the present time the framework varies somewhat by medium of exposure and by effect of exposure, in some instances by legislative mandate, and in others by differing beliefs of those responsible for the program. It certainly is the scientist's role to be interested in such problems and to supply a coherent and rational basis for the framework within which regulatory decision-making can proceed.

Certain other concepts are useful to

consider when discussing standard setting, such as threshold dose "safety," dose-response, extrapolation of data from animal to man, and combined effects.

Threshold Dose

A biological effect may not be observed until the dose reaches a certain level. This level is called the threshold dose, and it may be defined as the minimum dose required to produce a *detectable* effect. The concept is important from both a practical and a theoretical point of view, since a true threshold implies that below a given dose there is no adverse change whatsoever with regard to toxic effect of the substance studied; this allows a "safe" limit or standard to be specified. As one uses more sensitive indicators responses are identified at lower levels and the thresholds decrease. For example, illness frequently is a more sensitive measure of response than death, and both will be preceded by physiologic changes heralding the onset of disease. For many pollutants, such as ionizing radiation, there may be no threshold for the response.[1]

The existence of a threshold may be due in part to the build-up in man of tolerance to the particular pollutant. Tolerance represents the ability of man to endure pollutant exposures without apparent ill effects. The level of tolerance to environmental agents may be directly related to a number of characteristics including age, sex, and nutritional state. The concept of adaptation signifies an increase in tolerance with long-term low-level exposure to a given adverse environment. Adaptation is characteristically related to the stressful components of the environment. The ability to adapt varies in a population and is determined by anatomic, physiologic and biochemical characteristics of individual organisms.[1]

"Safety"

It is my present opinion that threshold concept is not a very helpful one because it is a level that changes with time, i.e., as measurement methods for either the pollutants or the biological indicator improve, the threshold will shift. Similarly a shift can come from an alteration in the conditions under which an experiment is performed. This shift in threshold can cause a shift in standard which in turn results in regulated industry attempting to control to a moving target. Furthermore, there are many pollutants where no threshold exists or else it seems to be negligibly low. The concept of threshold carries with it the implied assurance that there is a "safe" level of exposure. While this may occasionally be true, it certainly is not always true. Science recognizes no absolutes. Rather than think in the terms of safety in an absolute sense, ". . . our goal must be to reduce the risk (or probability) of hazard to a minimum consistent with the needs of our society. We must recognize that all human activity affects other humans, harmfully in some cases; our regulatory function is to evaluate how harmful activities under our regulations are, who is harmed, and how great are the benefits of permitting the activity to continue in spite of harmful effects. This latter has been frequently referred to as standard setting based upon evaluation of risks against benefits. During this transitional phase, it is important that all of our guidelines, criteria, etc. be stated in such a way as to make it clear that we are not expecting to achieve absolute safety to man or his environment (Elkins, 1974).

"It must also be recognized that we have a history of many years during which our scientists have attempted to accommodate to the popular (and sometimes Congressional) demand for safety.

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[1]Message from the President of the United States Transmitting the Report of the Department of Health, Education and Welfare and the Environmental Protection Agency on the Health Effects of Environmental Pollution, Pursuant to Title V of Public Law 91-515. 92d Congress, House of Representatives Document No. 92-241, February 1, 1972.

To do so they have developed definitions for words such as "safe level," "no effect level," and others that use quantifying adjectives such as "negligible," "trivial," "virtual," "insignificant," that tend to satisfy their intellectual recognition that absolutes do not exist in most common situations but still permit use of terms in such a way that the general public is given the implication of safety as an absolute. It is not possible for any one regulatory agency to avoid use of terms thus defined as long as their use remains well entrenched in the scientific and regulatory community." (Elkins, 1974).

In another personal communication, Lindsay[2] indicated that, "Estimates of 'safety' are expressions of ignorance. They are based on what we do not know."

Dose Response

Dose-response data extracted from either community or experimental studies is crucial for the decisions that must be made. However, when the information is available, as it rarely is, it has been gathered in animals at rather high exposure levels and needs to be extrapolated to man who will be exposed at lower levels. Man, however, will generally be exposed in greater numbers and for longer periods of time so that low-level risk can be manifest. We urgently need more specific research into the issues of extrapolation so that better decisions can be made. Since dose-response information is frequently lacking, other techniques need to be considered.

Extrapolation[3]

Studies of laboratory animals have been used to assure that the toxicity risk from an environmental chemical is acceptably low. Such assurance should be given before a chemical is introduced into the environment in a fashion where there can be human exposure. However, relating animal studies to possible effects in man poses difficult and practical problems. As more chemicals for which there is sufficient toxicity testing are introduced into the environment where man can have similar exposure, we can learn more about the validity of the testing. Until that time we rely on such information as ". . . cancers in laboratory animals are essentially the same as human cancers and with a single possible exception, all known human carcinogens are carcinogens in laboratory animals, laboratory animal studies seem to predict for man. Thus, for ethical and practical reasons, data derived from use of laboratory animals for toxicity testing is the foundation of efforts to protect the public from the possible harmful effects of new and old chemicals in the environment."

The normal sequence of animal tests of a new chemical agent begins with studies to determine the mechanism through which laboratory animals respond to a compound, the nature of metabolites, the distribution of the parent compound, the metabolism in the tissues and organs, and the rates and routes of elimination of the compound or its derivatives. Comparison of the similarity of the results of these tests can be made among various animal species, thus laying the ground work for prediction of the exposure events in man. Attempting to systematize this process is a prime area for research. Also, it must be remembered that this approach is most useful for observing effects that occur soon after the compound is administered, but is less useful for observing subtle long-term effects.

When attempting to bridge the gap between non-human and human toxicity one must recognize two steps involved in the extrapolation of laboratory toxicity data to man. First, the extrapolation of what might be called the average or median mouse to the average or median man. This consists of extrapolation from the average or even the most sensitive

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[2]Lindsay, D.: Personal communication, March, 1974.

[3]Weinhouse, S. and Rall, D.: Personal communication, January, 1974.

one, two or three laboratory animal species to an average man living under conditions with a commonly encountered genetic make-up. The second step requires identifying and allowing for variability in the human population. The median mouse to median man extrapolation is the easiest step. Biomedical research results are beginning to illuminate the differences and similarities between species and are beginning to provide a rational basis for extrapolation. The second step, allowing for variability and diversity—both genetic and environmental, is much harder.

In summary, there is a basis for comparison of the median mouse to the median rat to the median dog to the median man. More and more patterns that are useful for extrapolation to man are being recognized and can be identified in the course of the study of the pharmacological disposition of a chemical. Most of the differences that have been observed suggest that man is more sensitive than the usual experimental animal, and this must be kept in mind when developing safety factors.

The following principles were stated in the Weinhouse and Rall communication as this subject related to carcinogenesis testing:

1. The major principle that must be accepted if we are to deal at all with long-term assessments of toxicity in man is that effects in animals, properly qualified, are applicable to man.
2. Methodologies do not now exist to establish a safe threshold for long-term exposure to toxic agents.
3. The exposure of experimental animals to toxic agents in high doses is a necessary and valid method of assessing carcinogens hazard to man.
4. Agents should be assessed in terms of relative risks rather than as safe or unsafe.

Combined Pollutant Effects

Physical-chemical interactions occur among the pollutants in the environment which in turn alter the biological activity of the contaminants as well as the reactions within the tissues of man and animals. The resulting bioeffects may be synergistic, additive or antagonistic, resulting in a reaction whose magnitude is greater, equal to, or less than the sum of the individual constituents.

One of the most recent and best demonstrations of synergism of two pollutants was presented by Bates and Hazucha (October, 1973). The following is a quote from that paper:

"Our most recent experiments have been concerned with an additional problem, namely a possible interaction between ozone and SO_2. The episode of pollution in Rotterdam that occurred two years ago had several puzzling features. Although the ozone levels did not reach much beyond 0.2 ppm, and the SO_2 levels at the same time were about 0.2 ppm also, there was considerable morbidity, particularly amongst people bicycling in such an atmosphere. These levels of the individual constituents seemed to be too low to have caused the considerable symptoms which were reported. There is evidence that ozone and SO_2 together affect plants at lower concentrations than each would individually, and since nowadays ozone exposure and SO_2 exposure may be expected to occur together, it seemed most important to us to try and study the interaction of the two."

Fig. 5 (Bates and Hazucha, 1973) shows the effect on four measures of pulmonary function of 0.37 ppm of SO_2 and 0.37 ppm of ozone exposure independently, and then the enhancement of effects when both gases are present simultaneously. It is interesting to note that the effect of the two gases administered simultaneously is greater than the sum of the effect of each individually, thus a synergistic effect has been demonstrated that might account for episodes such as the one mentioned above and others both in U. S. A. and Japan.

Least Case and Worst Case Range Estimates (Shy, 1973)

Derivation of range estimates based on least case and worst case assumptions

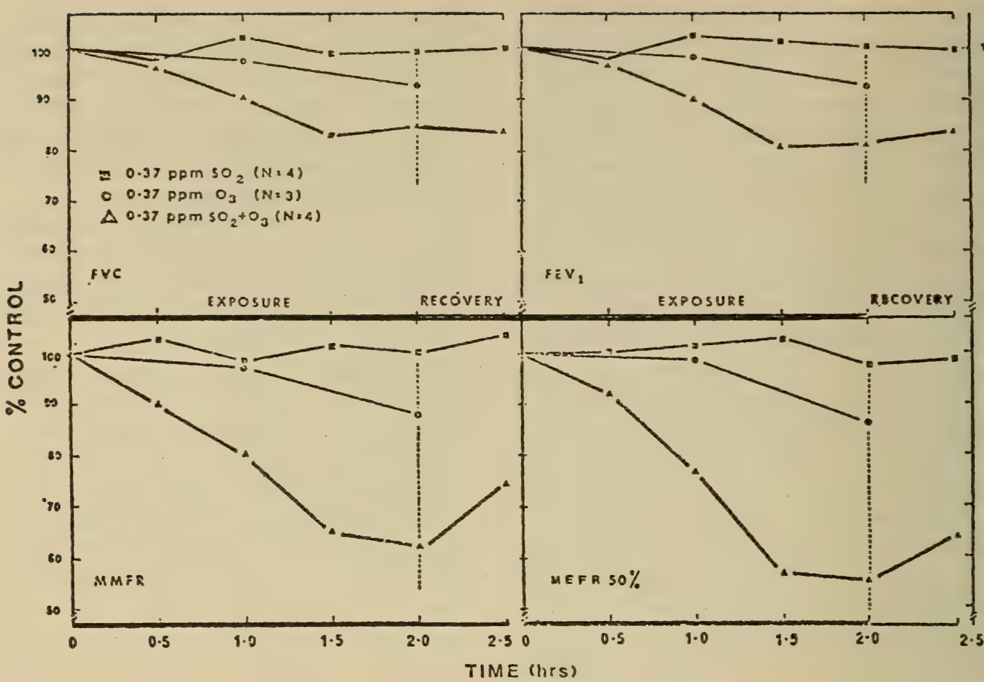

Fig. 5. Effect on 4 measures of pulmonary function of 0.37 parts per million of SO_2 and 0.37/ppm of ozone independently, and then the enhancement of effect when both gases are present simultaneously. (Based on Fig. 14, p. 534, of U. S. Congress Document No. 93-15).

is another way to develop a workable plan upon which action decisions can be based. In least case assumptions, quantitative assessment of exposure or response is made on the assumption that current exposures are not representative of long-term trends or that adverse health responses should first be attributed to all known covariates, and only residual excesses of illness frequency be attributed to pollutant exposure. For example, in the frequently encountered situation where high exposure and low socioeconomic status geographically concur, the effect of low economic level on illness frequency would be identified first. Any excess illness which could not be accounted for by economic level would be quantified as a residual effect. After all covariates were considered in turn, the final residual excess would be called an air pollution-related health effect. Least case estimates attribute the smallest possible effect to pollutant exposures, do not allow for interaction between covariates and exposure, and give a maximum quantitative estimate of human exposures associated with adverse responses.

Worst case assumptions attribute adverse health effects first only to covariates which are known to be strong determinants of disease frequency, such as cigarette smoking in relation to chronic bronchitis. But covariates which are not well founded as determinants of illness frequency are eliminated from final analysis, and air pollution exposure is assumed to have contributed to the relatively larger residual in excess illness frequency. Likewise, if information on past exposures is of low quality or unavailable, current exposures are assumed to represent past experience, and chronic disease frequencies are calculated as a function of exposures at current levels. Worst case assumptions, therefore, give minimum estimates of exposures associated with adverse health responses, and tend to maximize the

proportion of disease frequency attributable to pollutant exposure.

The truth may lie at either end of the quantitative range which can be derived from least case and worse case assumptions. When health intelligence cannot give precise quantitative information, the decision maker should be provided with least and worst case range estimates. At that point, the degree of control becomes a function of other policy considerations, including control costs, alternate control strategies (and the health effects of these), the severity or magnitude of the effect, the population at risk, etc. Failure to present range estimates leaves less room for control options and forces decisions based on one set of numbers derived from arbitrary interpretations about study results.

Least Cost Protective Standards (Shy, 1973)

Air quality criteria supply the existing rationale for air quality standards. The process of standard setting requires policy decisions concerning protection of public health at least cost to society. Sound quantitative exposure response data allows the policy maker to make clear-cut decisions which can be defended. When range estimates are broad due to inadequate health intelligence one of two possible kinds of error may be made. The resulting standards could be too lenient and not protective enough or they could be too strict and excessively costly for the real benefit derived. Thus, society must pay a price for not generating adequate environmental health information. It must also be remembered that generation of health information itself can be costly and that in some instances it may be less costly to overcontrol than develop the specific information required.

Margins of safety built into standards are judgements required to bridge the gap between the inadequacy of health intelligence and the need to stop continued exposure to hazardous pollutant levels. Greater degrees of uncertainty generate larger safety margins. Unfortunately control costs increase exponentially as exposure is limited more severely. The health benefit from the additional degree of control may be insignificant or actually negative if overly stringent controls cause health disbenefits, as could occur if scarce resources are diverted from health care or protection to unnecessary pollution abatement.

The function of a health intelligence system in the standard setting process can be graphically displayed in the form of marginal cost curves (Fig. 6). The true social cost of pollution is equal to the sum of the marginal cost of control and the health and welfare cost of exposure. Health costs of disease, and more especially of physiological dysfunction, cannot be readily estimated with current scientific knowledge. Health and welfare costs increase with higher exposures, while control costs increase as exposures are lowered. True social costs of pollution are minimal where the two marginal cost curves intersect. If society will tolerate some pollutant-associated excess health costs, curve AD (Fig. 6) may represent this social preference. On the other hand, if society places a very high premium on preventable disease or reducing risk of disease,

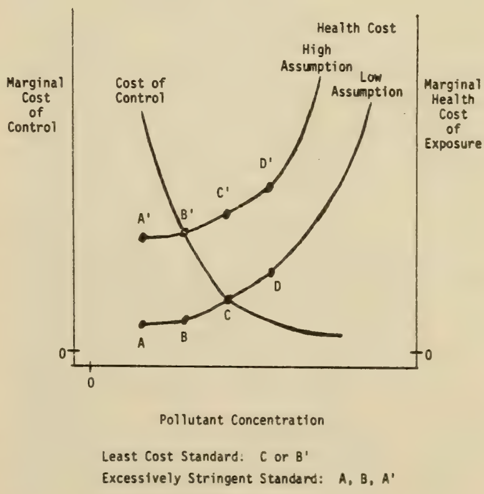

Fig. 6. Least cost protective standards.

curve A′D′ (Fig. 6) may well represent this attitude. On both curves, the points B or B′ represent the lowest pollutant exposure associated with adverse health (or welfare) effects. Under the low health cost assumption (curve AD), society will accept the health costs (and associated adverse health effects) represented by the vertical distance between B and C. Under the high cost assumption, society will not allow any adverse health effect caused by exposure and will require control to point B′, where high marginal cost of control is equated with derived health benefit.

Excessively stringent standards are shown at point A and B under the low cost assumption and at point A′ under the high assumption, while too lenient standards are reflected in point D under the low assumption and C′ and D′ under the high assumption. The least cost standard, in terms of true social costs, is at point C under the low health cost assumption and at point B′ under the high assumption. Least cost standards protective of health require adequate quantitative exposure-response information, knowledge of control costs and range estimates of health cost of pollution effects.

Even range estimates of pollutant-related health costs are difficult to derive. Many direct and indirect health costs must be considered, including: Immediate and delayed health effects of short-term and long-term exposure; the contribution of pollutant exposure to the occurrence and severity of major public health problems such as acute and chronic respiratory diseases, heart disease, cancer and congenital defects; and adverse health effects of control strategies. While these estimates are formidable tasks, failure to make the estimates will prevent even the possibility of achieving least social cost standards, except entirely fortuitously. Required annual reports of air pollution control costs clearly warrant better and more quantitative health effects data and concerted efforts to convert health effects to health costs.

Implementation and Enforcement

Implementation is the translation of a control regulation into an action program that hopefully will achieve the prescribed goal (standard) within a designated time-frame. Enforcement is policing of the system and bringing action against offenders who do not meet the requirements of specific emission or effluent standards as deemed necessary under the implementation plan. Each of these steps in the process has many statistical problems.

Ambient Standards vs. Emission Standards

Ambient air quality standards are difficult to enforce for if such an air standard is exceeded, who is to blame? It is a community standard and surely the whole community is not to be punished. However, since pollutants come from specific emittors, an effort is made during development of an implementation plan to establish a set of emission standards that, when integrated to the monitoring sites for the specified time periods, will keep the measured levels of pollutants below the ambient air quality standards. Since there are many pollutant sources at varying distances from the measurement sites and since there are variations in measured levels depending on the sampler, the day of week, and other factors, it is not difficult to speculate about a host of statistical problems. The situation is, of course, even more difficult because standards, or legal limits, are expressed in rather absolute terms, 75 ug/m^3 as an annual average or 0.08 ppm for 1 hr not to be exceeded more than 1 time annually. These numbers are or are near upper bound values and never give any indication of the magnitude of the variation in the system that arrived at the number. Problems arise when the implementation plans are being evaluated for adequacy or when an enforcement

action is to be taken. A clear statement of the variability, or at least some components of it, would provide a sounder basis for evaluation.

I do not wish to give the impression that there are no mathematical models relating ambient air quality and emission levels. Such models are available, and are still in need of refinement. Furthermore, information is available to place confidence intervals around the air standards or at least to indicate what mean value the upper bounds designated as standards actually imply.

Assessment of the Action Program

One activity frequently forgotten in public programs is evaluation to learn if the program does or does not achieve its desired end. Environmental pollution control programs are assessed in two ways: (1) One in terms of monitoring pollutant levels and (2) the other in terms of community surveillance to learn if the health and welfare improvement desired is achieved. The first method is the least expensive way to evaluate the program, but has the disadvantage that the benefits sought are assumed rather than measured and new problems that might arise because of the program are not identified until much later. Since health surveillance can both indicate achievement of the program and identify new or continuing problems simultaneously, it is an important part of the regulatory program.

Summary

The regulatory control process was schematized into four steps, namely: Environmental problem identification and assessment, alternate control strategies, implementation and enforcement and assessment of the action program. These steps in the control process were used to discuss issues in health research and use of the data for decision-making, such as: Public sector vs. private sector responsibility for research support; need for an integrated program using the disciplines of epidemiology, clinical research and toxicology; pollutant exposure and human response emphasizing the spectrum of response, the exposure response matrix, estimates of exposure and evaluation of the exposure response relationship; health protection vs. control costs; environmental standards; threshold dose; "safety;" dose-response; extrapolation; combined pollutant effects; least case-worst case range estimates; and least cost protective standards.

The role of the scientist as viewed by me is to unravel and develop understanding of the environmental problems. This understanding should be used to provide judgements that will bound the problem for the administrator/politician decision-maker as well as supply a coherent and rational basis for the framework within which regulatory decisions can be made. There is room for much original and innovative thinking in this area.

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Keynote Address: Statistics and the Environment

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"Keynote speech" is defined by Webster as "a speech, as at a political convention, that sets forth the main line of policy." While that is an admirable definition when visualizing the political process, the definition seems strangely misplaced here. Not only would it be presumptuous of me to attempt to set policy for the organizations represented here, but this gathering hardly resembles a political convention. There are no banners, there is no music, the ambient level of smoke in this room seems rather low, and I doubt if any votes are to be taken.

Scientists rarely engage in any of the ballyhoo associated with the political process. The rewards of scientific endeavor are found largely in the endeavor itself and in the recognition of accomplishment by one's peers. Certainly those rewards are honorable and have played an enormous role in fostering scientific advances throughout history.

But I wonder if the image of the scientist tucked away in his laboratory, speaking a language often known only to himself and his peers, should not be changed and changed dramatically. Quite frankly, I find myself longing to attend a scientific meeting that more closely resembled a political convention. While obviously a symposium like this is no place for much of what goes on every fourth year, political conventions do serve the very important function of rallying members of each party around a central theme. In that respect, perhaps a bit more of the political convention atmosphere might be in order in scientific meetings. And if I were to choose a theme to rally behind, it is that the scientific community must strive to make itself more visible and available to policy makers than it has in the past. My specific frame of reference is the Congress and that is the essence of the theme I would like to develop this morning. The Congress has a pressing need for solid scientific advice, and it has been all too hard to get in the past.

Before developing this theme in more detail, it might be helpful to describe my role on the staff of the Committee on Commerce. Perhaps then it might be easier to understand my feelings about good scientific advice and its role in the legislative process.

My job is primarily to offer technical advice which is relevant to the formulation of regulatory policy on certain environmental matters. Stated differently, it is my function to attempt to under-

stand environmental threats and then to translate this understanding into legislative language that hopefully will provide appropriate remedies. My job is not substantially different from those of other staff members on the hill. Identifying problems and proposing legislative remedies is a function shared by most committee staffers, although my area of specialty probably requires a greater understanding of scientific principle than others.

In one respect, however, my perspective differs substantially from that of many of my peers on the hill. I do have a degree in fish and wildlife biology and engaged in that profession for a number of years before joining the staff of the Committee on Commerce.

This does give me a certain uniqueness which is not at all unwelcome. At certain times, however, I find this piece of personal history to be more a hindrance than a benefit. Unfortunately, many of my lawyer peers regard anyone who might even remotely be termed a "scientist" an automatic expert on everything from thermodynamics to biostatistics, both of which, incidentally, I have been called upon to speak in the past. I would find this tale somewhat amusing were it not for the fact that it illustrates a very serious lack of technical expertise available to Congress.

As we all know, a great deal of legislation to protect the environment has been proposed and enacted in the past few years. Far-reaching legislation to protect our air and water resources has become law, as has tighter control over noise, radiation, ocean dumping, and the protection of other components of the living environment. In each case, and I really am not aware of any exception to this rule, the enactment of a statue has occurred only after scientific facts or alleged facts have sounded the alarm. The death of Lake Erie and the disastrous effect on biological systems of the polluted waters of the Houston ship channel and the Cuyahoga River created strong pressures for the enactment of a stiff water pollution control law. The effects, or potential effects, of air pollution in the smog-filled Los Angeles Basin and Washington, D. C. for that matter, created strong motivation for the enactment of the Clean Air Act amendments of 1970. Within my sphere of responsibility, the discoveries of polychlorinated biphenyls in edible chicken and the effects of phosphates on aquatic eutrophication have provided much of the impetus necessary for Congress to focus attention on the Toxic Substances Control Act, which, hopefully, will become law in the near future. Likewise, a survey conducted by EPA entitled "The Community Water Supply Study" has provided much of the ammunition to shepherd the Safe Drinking Water Act through the Senate.

The common thread among all of those examples is that each of them requires at least a rudimentary understanding of the effects of pollutants on biological systems.

Obviously, the importance of scientific input goes far beyond the bounds of environmental legislation. Health legislation, foreign affairs, housing, drug abuse, agriculture, fiscal and monetary policy, and many other areas of legislative endeavor would be doomed were it not for the lynch pin of technical input at some point in the legislative process.

The formulation of scientific fact and its translation into terms laymen can understand is a fundamental need of an aggressive Congress. Much of the reluctance that we find in Congress to developing specific policy directives in matters of science results from a lack of understanding of the scientific principles involved. For example, a key issue for the House and Senate Conferees on the Emergency Energy Act was the degree of discretion to be given to the President to impose emergency energy conservation measures. If better information were available to the Congress on the effectiveness of the various measures contemplated, one can legitimately question whether the issue of how much power be given to the President might cease to be an issue at all as Congress

would take the initiative. To carry the principle to its logical conclusion, might not the lack of technical input and understanding of technical information by the Congress be a prime factor leading to the very substantial transfer of authority from Congress to the Executive Branch in recent years.

It is perhaps unfortunate that often the predominant scientific input to the legislative process comes from those who are most vociferous. While assertiveness is an admirable quality, the essential ingredient of impeccable scientific credentials is too often difficult to discern. As many of you know, it is a staff responsibility to seek out witnesses for Congressional hearings. In structuring hearings involving matters of scientific principle, there is no more difficult task than finding respected scientists who can speak on an issue forcibly and in layman's terms. The frustration becomes overwhelming after supposedly having found such a witness and listening to thirty minutes of excellent scientific testimony, the Chairman of the hearing turns to the staffer at his elbow and asks under his breath, "What the hell is he saying?"

Lest these comments be interpreted as undue criticism of the scientific testimony we do receive, I have nothing but admiration for those scientists who volunteer time and time again to offer testimony to the Committee. Despite this, however, all too frequently we are forced to call upon the same witnesses to address themselves to a variety of issues, some of which they are obviously the more qualified to speak to than others.

The disparity in the amount and types of technical support between the Congress and the Executive Branch is indeed staggering. For example, Dr. Stanley Greenfield has nearly 2,000 employees at his disposal to carry out the research and monitoring functions of the Environmental Protection Agency, one of the smaller agencies of the Executive Branch. In fact, over a quarter of a million persons are employed in technical positions in the entire Executive Branch. On the other hand, the standing committees of the Congress, who are responsible in large part for escorting legislation through the legislative process, employ approximately 1,500 people. Obviously, the duties of the Legislative and Executive Branches are not comparable. But there is little wonder in my mind as to why the support of the Administration is so very important in passing legislation which requires scientific understanding. Quite frankly, we are unmercifully outgunned.

Obviously, there are some institutional changes which Congress must consider to narrow the technology gap. In fact, a number of changes are already evident. As many of you know, the Congressional Research Service of the Library of Congress has long provided technical research service to members of Congress. Their staff is highly overtaxed, however, and emergency requests can rarely be honored.

The Congress has established an Office of Technology Assessment within the Library of Congress. The purpose of OTA, now in its formative stages, is to aid Congress "in the identification and consideration of existing and probable impacts of technological application," obviously a vital service.

The General Accounting Office, Congress' so-called watchdog agency, is made up largely of technical experts whose function it is to audit government programs which many times are technical in nature. Again, a vital function.

On the non-governmental side, there is evidence that scientific and professional organizations are gradually turning their attention to the Congress. The American Association for the Advancement of Science (AAAS) sponsors several Congressional fellows each year as does the American Society of Mechanical Engineers (ASME), the American Physical Society (APS), and the Institute of Electrical and Electronic Engineers (IEEE). The Committee on Commerce was blessed to have the first such

fellow, Dr. Barry Hyman of George Washington University, assigned to the Committee this past year. Dr. Hyman played a substantial role in the Committee's consideration of the National Fuels and Energy Conservation Act and other energy legislation. Dr. Hyman has agreed to join the staff for an additional year and to assume staff responsibility for the Subcommittee on Science, Technology, and Commerce.

Obviously, the prime responsibility for obtaining technical information pertaining to legislation must lie with the Congress. But should the responsibility end there? What should be the role of the scientific and professional organizations like many of those sponsoring this symposium? And how about the role of the National Academy of Sciences, whose name has become synonymous with scientific excellence in this country, at least in most circles. Is there not a responsibility to make your voices heard loud and clear in legislative matters involving science? And I am speaking about a great deal more than lobbying to keep research budgets at such and such a level, although that role obviously is vital. I am talking about taking some lessons from the public interest movement and aggressively involving yourselves throughout the legislative process in matters ranging from the regulation of the chemical industry, to occupational safety and health, and perhaps more to social issues which bear on science, like the manner in which the fruits of science (like certain pesticides) are to be used in warfare. Obviously the list of potential legislative matters in which you could involve yourselves is very long.

Keeping abreast of Congressional activity and offering your services not only to those who actively seek help, but to those who might reluctantly accept it, can only foster a greater understanding within the Congress of science and scientists. For the scientific and professional organizations, this could well involve staffing a national office here in Washington as some have recently done and employing sufficient competent lobbyists and staff to make your point abundantly clear. It is a difficult, often unrewarding task, but one which stands to yield substantial benefits.

To complete this exhortation, let me depart from a promise I made at the outset of this talk, that of not being presumptuous enough to attempt to set policy for this symposium. As you continue for the next three days and after you go back home, I would hope that each of you would continually ask the question, "Do I have knowledge that has legislative application and might it help to set policy if it were known to the Congress?" If you decide in the affirmative, please let us know.

Statistics and the Environment

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The Problem

It seems only a little time ago that we were concerned with matters which now seem comparatively trivial. We had for some time lived with the knowledge that our survival was threatened by nuclear attack by a foreign enemy, but it seems only recently that we have noticed a more insidious threat of our own making. Most of us now recognize that we are well on the way to destroying ourselves by overpopulation, pollution, the frittering away of our raw materials, and the poisoning of our food by inadequately tested chemicals.

Opinions differ as to how long it will take before various predictable crises occur and how much each problem will complicate the solution of the others, but it is very clear that we will be hard pressed and we will be lucky to escape by the skin of our teeth. The truth is that although we are called on to meet very difficult problems of great urgency we know pathetically little of the facts. So we must learn fast.

Now it is precisely this ability to learn fast that has got us into our present difficulties. It was only a few hundred years ago that men's minds seriously turned to the question of how the, normally very slow, process of learning by chance experience might be accelerated. Scientific method, the secret of learning fast, has altered the normal birth and death process, yielding perhaps a more comfortable world but at the cost of world overpopulation. Scientific method has provided us with motor cars and factories producing convenient products, but the by-products of both are threatening the air we breathe and the water we drink. Furthermore, their insatiable appetite for raw materials is stripping the earth of its irreplaceable treasure. Scientific method has provided us with conveniently packaged foods with chemical additives which make them taste good, look good, and last a long time on the shelves of the supermarket, but pharmacologists will tell you that it is almost impossible to keep up with the flood of these new substances which we ingest, and to be sure what are their long term effects on human beings.

So we are hopeful that the same scientific method which has in a period of a few hundred years got us to where we are now, can in a few decades get us to where we would like to be.

I believe it can, but with two provisos:

First, we must release, by public education, the will to make it happen.

Second, because with so little time we cannot afford inefficient investigation, we must catalyze the learning process still further. The catalyst is the proper use of Statistical Methods.

Science and Statistics

It was Lord Kelvin who said, "When you can measure what you are speaking about and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of science." But, in case that should seem too much an encouragement to those who believe that mere unthinking accumulation of numbers is synonymous with good science

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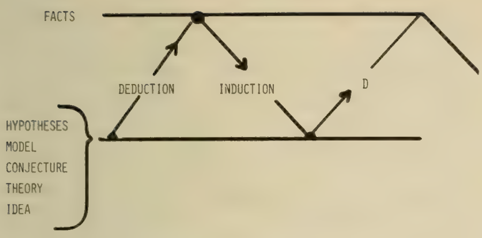

Fig. 1. The iterative learning process.

and will of itself solve the problem, I hasten to add the well known words attributed, among others, to Mark Twain, a contemporary of the noble lord's: "There are three kinds of lies—lies, damn lies, and Statistics."

What then is scientific method and what part does Statistics play within it?

Scientific method is a process of controlled learning. The object of statistical method is to make that learning process as efficient as possible.

Learning is an iterative process, illustrated in Fig. 1, in which a hypothesis (or theory or model or conjecture) leads by a process of deduction to certain consequences which may be compared with known facts. Usually the consequences and the facts fail to agree, leading by a process called induction to modification of the hypothesis. Thus a second iteration is initiated, the consequences of the modified hypothesis are worked out and again compared with facts (old or newly acquired) which, with luck, leads to further modification and to further gaining of knowledge.

This process of learning can be thought of in terms of the feedback loop shown in Fig. 2, where discrepancy between the facts and the consequences of the initial hypothesis H leads to the modified hypothesis H′. This view makes it clear why there is no place in science for the man who wants to demonstrate that he has always been right. For it is by arranging matters so that there is maximum opportunity to find out where he may be wrong, that most progress is made.

Suppose at a certain stage in an investigation the situation is that shown in the bottom half of Fig. 3. A hypothesis H concerning the state of nature has been formulated, leading to certain consequences that have been compared with the facts deduced from analysis of the available data. Discrepancies have suggested a modification from H to H′. Consequences of H′ may now be in accord with the data analysis or may still be discordant. When it is not clear what modification should be made to an unsatisfactory hypothesis or, alternatively, when confirmation of an apparently satisfactory hypothesis is needed, further data must be sought. Depending on the context, the further data may come from a designed experiment, a sample survey, or already existing results. Whatever the source of the data, careful attention must be given to its selection or design. As illustrated in Fig. 3, the direction of the effort at data getting will inevitably depend on our latest view of the state of nature and the hopes and fears which surround that view.

While, at a particular stage, the conjectured state of nature may be false or at least inexact, the data themselves are generated by the true state of nature. It is because of this that the comparison of

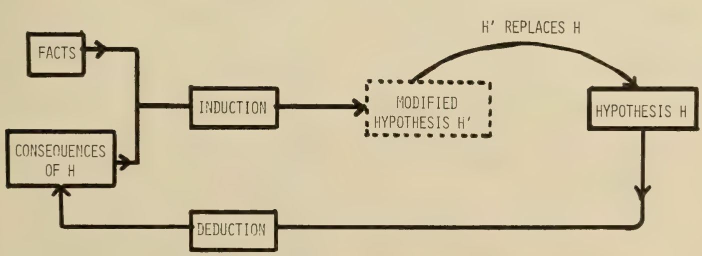

Fig. 2. The learning process as a feedback loop.

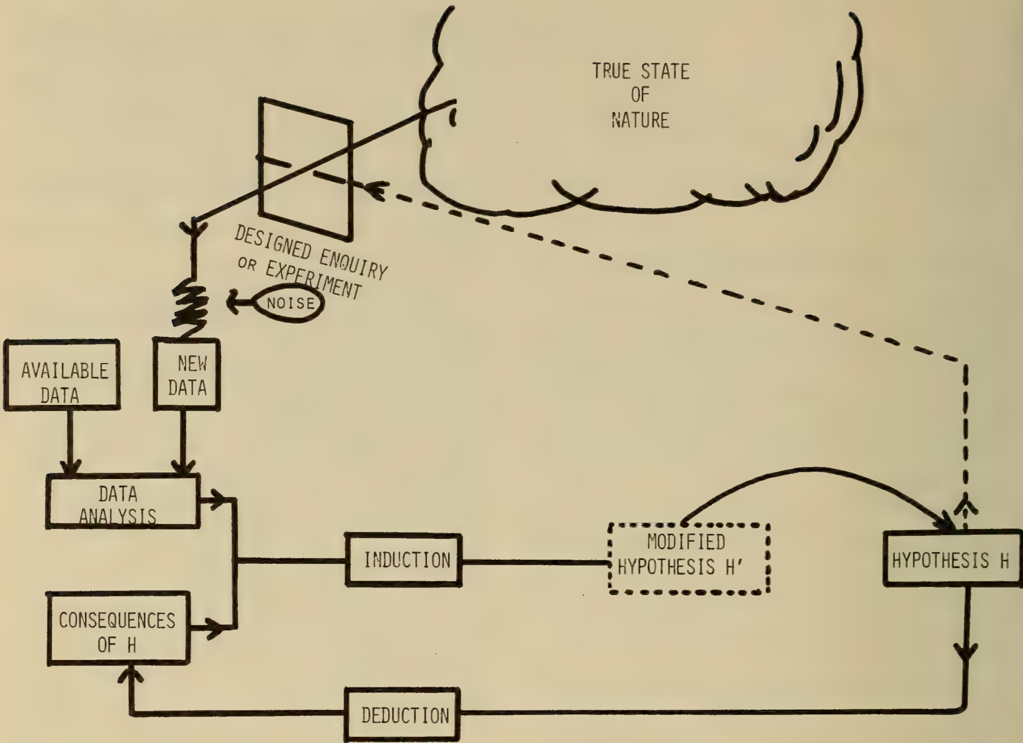

Fig. 3. Data analysis and data getting in the process of scientific investigation.

successively conjectured states of nature with actual data can lead to convergence on the truth. Even if we could see such data free of experimental error, however, the task of discovery would usually not be easy because of the complexity of the systems that need to be studied. So in practice in addition to complexity, we have to cope with an added difficulty—that the data contains experimental error (or noise), which tends to mislead.

Scientific investigation, then, is not easy, and obviously the process we have described depends crucially on the scientific wit and subject matter knowledge of the investigator. The statistician's job is to advise and assist the investigator in two crucial tasks, so as to allow the investigator to employ his talents most efficiently. These tasks are:

(1) deciding what would be appropriate data to get at each stage of the investigation. Broadly we can call this the *design* problem.

(2) deciding what the data entitles us to believe at each stage of the investigation. We can call this the *analysis* problem.

Of the two, *design*—the decision as to what are the appropriate data to get—is of paramount importance. This is equally true whether by actual design of an appropriate experiment, the planning of a suitable sample survey, or the proper choice of a data base. No amount of skill in data analysis can extract information which is not there to begin with. The second task of the statistician, although not so vital as the first, is still very important. Inappropriate analysis of data can produce unjustifiable conclusions or fail to discover justifiable ones. *Worse,* it can fail to unearth those hints of, perhaps unexpected, phenomena which often catalyze the investigator's progress to a solution. In any case, inappropriate analysis

of data will greatly hamper convergence of the scientific iteration.

In summary then, we learn through numbers. But what numbers or data should we try to get and what do they mean when we have them? These are the questions that good statisticians are trained to answer. It is very easy to acquire useless or irrelevant data. It is very easy to be misled by data once they are acquired. The design of *each stage* of an enquiry so as to produce useful data with the minimum of time and expense, and the analysis of data of each stage so as to produce, not only valid conclusions, but also valuable hints on how the investigation ought to proceed, these are the two critical tasks in which the statistician plays a key role.

Part of the statistician's job is also, I think, to encourage and accompany the scientist in the slightly schizophrenic role that he has perforce to play.

Having entertained a tentative model (hypothesis, etc.) it is up to the statistician to see that fully efficient means are used to investigate the consequences of that model. That is the inference step in Fig. 4. However, having then produced the best analysis possible, supposing the model to be accurate, he must now change his stance from that of a sponsor to a critic. He becomes a doubting Thomas prepared to find fault by inspecting residuals for suspicious features, etc. This criticism can lead to modification of the model, either at once or at some time after more data has been taken.

Switching alternately from sponsor to critic and back again is a painful business but one which we must steel ourselves to pursue. The Pygmalions who have fallen in love with their models somewhere along the way are a nuisance and a hindrance to progress.

Another part of the statistician's job is to make sure that Statistics and Computers do not separate the investigator from his data but, on the contrary, help him to see his data from many different angles. We must remember that the best induction machine so far devised is the human mind, and if modern methods of dealing with data result in separating the investigator from his data, they are almost certainly doing more harm than good.

Going now into a little more detail, what then are some of the difficulties that appropriate use of statistical methods can alleviate or avoid?

Coping with Natural Variation

We live in a world which is universally variable. How much air a man breathes depends on the particular man, his temporary physiological state, the atmosphere he is presently in, and so forth. And yet, until quite recently, attempts were made to study variable phenomena in an entirely deterministic manner. Variation was frowned upon, as if disapproval

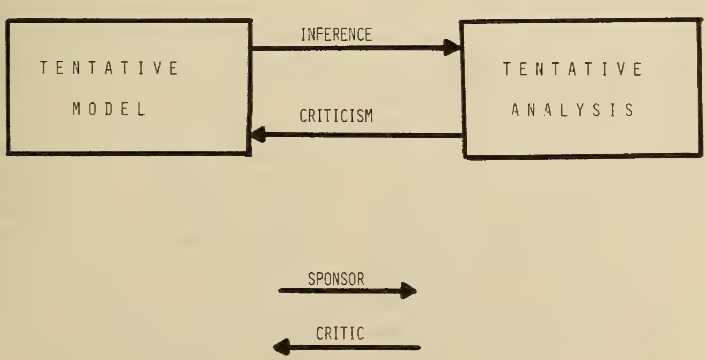

Fig. 4. Statistical analysis as an iterative process.

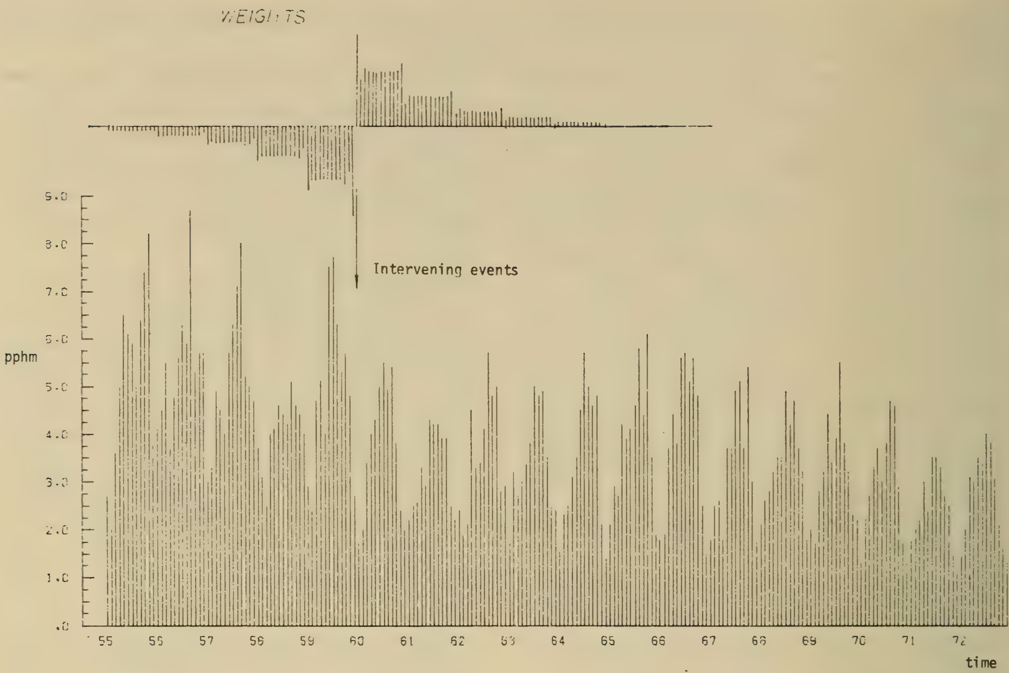

Fig. 5. Monthly average of hourly readings of O_3 (pphm) at downtown Los Angeles (1955–1972), with the weight function for estimating the effect of intervening events in 1960.

would make it go away, and probability statements were treated as in some way unsatisfactory. There was little readiness to admit that everything varies and, except perhaps from God himself, every statement, if exactly made, would have to be a probability statement.

Increasing Accuracy by Exploiting the Variational Structure

Environmental data are usually highly variable. It is by facing this fact, rather than running away from it, that we can solve some of our problems. Indeed, it is a fascinating fact, that it is the *structure* of the variation or noise, which determines how we can extract the information which the data contain. As an illustration, Fig. 5 shows monthly averages of oxidant (O_3) levels observed in downtown Los Angeles from 1955 to 1972. These data are highly seasonal and variable. About the beginning of 1960 two events occurred which might have been expected to change these levels. These events were the diversion of traffic by the opening of the Golden State Freeway and the coming into effect of a new law (Rule 63), which reduced the allowable proportion of reactive hydrocarbons in the gasoline sold locally. By a study of the structure of the variation it is possible to obtain (Box and Jenkins, 1970; Box and Tiao, 1965, 1973; Tiao et al., 1973) a valid and most sensitive test of the possibility that the events in January 1960 changed the oxidant level and to estimate the change. For this data the estimate turns out to be -1.10 ± 0.10 p.p.h.m. The function shown at the top of the diagram displays the manner in which the data are weighted in the optimal difference estimate. As common sense might expect (i) most weight is given to values obtained immediately before and after the events and remote data are suitably discounted, (ii) the weighting is automatically chosen so that seasonal effects are eliminated.

I believe that the use of "Intervention

Analysis" such as the above, in which difference equation models are used to represent dynamic and stochastic systems, has much to contribute in uncovering possible effects of public policy changes. For example, it could show the effect of the opening of a nuclear power station on the ecology of the river from which cooling water is drawn and returned. It is clear that studies of this kind are vital to the intelligent framing of new laws.

We owe to Sir Ronald Fisher the concept that we can exploit the patterns of natural variation in data to *design* enquiries and experiments so that errors are minimized. For example, randomized block designs and stratified sampling plans can eliminate major sources of disturbance and ensure that important comparisons are made within the least variable material.

Another tool which should, I believe, find much application is the use of components of variance to improve tests of environmental quality. The analysis of variance table used in the analysis of data from the randomized block designs I mentioned above may also be employed in conjunction with suitable designs to estimate components of variation, for example, in tests of environmental quality. Suppose we take a sample from a stream and perform an analysis. How accurate is the result we get? What do we mean by that question? Certainly not how closely repeated chemical analyses of that same sample would agree with one another. What we want to know is how nearly does our analysis give a picture of the quality of that stream at that time and place.

An appropriate study of components of variance—how much variation is associated with chemical analysis, how much with the sampling method, how much with change of location in the river, together with knowledge of how much it will cost to take a sample and perform a chemical analysis—enables us to devise a testing scheme which can be dramatically more accurate and economical than one naively chosen.

Causation and Correlation

Many years ago when I studied statistics at University College London there was a plot of some data which none who saw could easily forget. On the x axis was the number of storks' nests observed each year in a certain town; on the y axis was the corresponding human birth rate for that year. The data showed an almost perfect straight line relationship. It is perhaps superfluous to explain that the correlation arose because, over the period of years in which the data were taken, the stork population was increasing and so was the human population. It is also unnecessary to point out that our over-population problem will not be solved by shooting storks.

In case these remarks should seem frivolous we must remember that it was precisely this kind of question which was debated in some of the early discussions on smoking and lung cancer and which bedevil much data analysis in other fields.

Again it was Fisher who showed how in planned experimentation the introduction of randomization could break the purely correlative chain and enable causation to be distinguished. In cases where planned experiments are not possible the situation is always very tricky, and very careful analysis is needed to decide in any given case precisely what the data allow us to conclude.

Complexity in the Face of High Noise Levels

Many of the phenomena we face in considering the environment are complex. To cope with problems which are complex, as well as being obscured by experimental error, we would be wise to welcome whatever help we can get. Even though the complexity of problems is admitted, the idea that variables should only be studied one at a time dies hard. The one variable at a time method would, of course, only be a satisfactory mode of

study if nature were so obliging as to have its variables affect the environment independently. Again, it was Fisher who pointed out that by the use of suitable design the effect of experimental error could be averaged out at the same time that provision was made for the estimation of complex effects. Designs of this kind may be used, not only for empirical descriptions of phenomena, but also for testing mechanisms. This is done by treating as data the estimated constants of the system. If the model is correct, these should remain constant when extraneous conditions are varied. When, as is usually the case initially, the model is not wholly correct, analysis of the changes in the "constants" provides a valuable diagnostic tool for model testing, pointing to where the model needs attention. Endelman (1973) has recently used these methods at Wisconsin to study nitrogen changes in the soil and soil water. In many ways this study was a model one, in which the Departments of Soil Science, Chemical Engineering, and Statistics all cooperated.

While on the subject of complexity a word should be said about the models needed to represent complex phenomena. In any given investigation it seems to me we can err in two ways. We can have too simple a model or too elaborate a model. My recent experience has been that investigators have often erred in building models that are too elaborate. There is a tendency to try to model each step that the investigator can imagine, whether there is strong evidence that that step really occurs in the system or not, whether the step affects the solution or not, and whether the data could possibly supply any information about that step or not. Even if he had a 50% chance of being right about any given step, the investigator need only introduce a few such steps into a system and the chance of error becomes overwhelming. My experience is that we must borrow William of Occam's razor and use it rather ruthlessly to remove deadwood. Usually, models are best built up from simple beginnings, elaboration being introduced only as it is shown to be necessary by actual comparison with data, as in Fig. 3.

The Peril of the Open Loop

Perhaps of all the problems that face us, whether personal, professional, scientific or statistical, the most menacing of all is the danger of the open loop.

I have spoken of the process of scientific learning in terms of a feedback loop. If the loop is open, learning stops, of course. The idea applies more generally. As an earlier speaker has so ably pointed out, feedback is essential between scientists and legislators; otherwise, even when the scientists know what to do, it cannot get done.

As another example, I recently attended a seminar where the speaker was building a pollution model for a city. The method he used was to calculate by dead reckoning the amount of every substance going into the atmosphere over each small area of the city. For example, he could calculate over, say, a given hundred yards square area, how much rubber was worn off the tires of automobiles passing through that area and hence presumably going into the atmosphere. There was nothing wrong with that, but I was surprised to hear him explain, as he commenced his seminar, that there were two kinds of modelling—his kind based on dead reckoning and statistical modelling based on data. Learning happens surely only when the loop is closed and what can be calculated from dead reckoning is compared with what the data actually say.

The Supply of Competent Statisticians

Perhaps finally I should say something about the supply of statisticians. A little while ago I saw a report prepared by a distinguished panel of mathematicians on the current need for graduate training in mathematics and mathematically related subjects. One conclusion was that since a principal outlet for Ph.D. mathematicians was as university teachers and since the great expansion of the universities had now ceased, we must plan for a

major cut-back in the production of Ph.D.'s or face the possibility of producing a glut of unemployed mathematicians. I was alarmed because the "mathematically related subjects" which the report claimed to cover included statistics!

Now whatever may be true about the future need for pure mathematicians, the fact is that we face a scarcity of trained statisticians competent to deal with real problems. Furthermore as, one by one, the various environmental crises become more obviously imminent and the need for hard facts on which to take sensible action becomes inescapable, the demand for such people will markedly increase. It takes many years to produce a properly trained statistician. It cannot be over-emphasized that steps must be taken now not to restrict but to expand the educational facilities available for the training of competent statisticians.

How do we get competent statisticians? Neither surely by producing mere theorem provers nor mere users of a cook book. A proper balance of theory and practice is needed and, most important, statisticians must learn how to be good scientists, a talent which, I think, has to be learned by example. At Wisconsin, we have taken a number of steps to help this along:

- To obtain any graduate degree, a student must have spent a period of time in the Statistical Consulting Lab working with the statistician in residence and other faculty to deal with clients' problems. This counts as a course for credit, and no student can graduate without passing this course.
- The Masters Degree, which all students are encouraged to take, whether or not they proceed to a Ph.D., is not a "failed Ph.D." degree but is awarded on their demonstrated competence to becoming a practicing statistician.
- A Monday night beer session is held in the basement of my house where research problems are discussed on an ongoing basis.
- The department is deliberately diversified with joint appointments and research interests in engineering, business, medicine and agriculture.
- Students act as research assistants in projects such as the Analysis of the Los Angeles Air Pollution data, the improvement of operating methods for the local sewage works, etc.

When we look at the history of the subject of statistics itself, there is no doubt that it develops most rapidly when there is active feedback, with practical problems initiating new theory and new theory in turn showing new ways to handle real situations. I believe we are moving now into a period of great statistical activity where, because of the service it will render to the community, our science will come into its own. In doing so, it will inevitably undergo new and exciting development.

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Keynote Session Discussion

Moderator: **Mr. Ralph C. Wands, National Academy of Sciences**

ELLIOT HARRIS (NIOSH)—I would like to ask a question of Dr. Newill. Dr. Newill rejects the threshold concept for a variety of reasons and believes that the cost benefit approach probably would be the best, selecting a population which would be at risk. I would like to know how he proposes to select that risk population. Would it be based on age, or upon the severity of the response, or perhaps upon the potential of productivity?

DR. NEWILL—I usually think about these problems more in terms of the general population than I do just the occupational population. I think that the same kind of rules would apply. The population that I try to get at in the general population is that group which is most susceptible. This means that I am not limited to a specific age range, or I can go through the whole set of covariance. I try to identify those people who are most likely to respond. That is where I would like to look for the effects, and then starting with the risk in that population, work back from there.

MR. WANDS—Dr. Newill, would you be saying, then, that the occupational population is the most vulnerable, not particularly because of its greater susceptibility but because of its greater potential exposure?

DR. NEWILL—In many situations this is true. In fact, for many things the only data that we have available are ones that come from the study of occupational populations. I believe that dealing even with general populations this is where we should start—to look at the occupational health data. But if we find no effects among people that are exposed in occupational settings, then I think we have to turn to the general public. I think that all positive information that comes out of the occupational area is useful. I think most of the negative information from the occupational health situation needs to be looked at very critically and tested in the general population before we accept it as a negative, because it does eliminate many of the susceptibles from the population.

Q—At the Academy session here on energy and fuels, one of the representatives from Johns Hopkins Public Health Department mentioned the fact that he was much more concerned about the air pollution that arose within dwellings resulting from faulty adjustment of fuel burning apparatus than he was about what came from the outside. Now it is curious to me that in all this discussion is there any discussion from the environmentalist? I would like Dr. Newill to comment on where we find the areas in which the most good can be accomplished from a relatively small expenditure.

MR. WANDS—Dr. Newill, this fits into the graph which you showed—cost versus degree of health protection.

DR. NEWILL—One of the problems that we have is that nobody has the overall responsibilities. The holistic approach is very difficult because it has always been fragmented, even though it's better now than it was a few years ago. You mentioned the exposures that can occur indoors. These are very real and as far as I know now there is no group in the Federal Government that has a responsibility for indoor exposure. I would agree with you that there are effects that can come from this, certainly around the cooking of foods if nothing more. We have measured levels of nitro-

gen oxides where people use natural gas for cooking and find that the levels to which they are exposed are in fact higher than the levels outside of the home, so that there are times when you could get the greatest benefit by paying attention to this. All I would plead for is that we have to look at this kind of thing systematically across the whole range and apply our money where we are getting the most effects. I don't think we have a good integrated system of doing that at the present time.

MR. WANDS—There is one opportunity for controlling the exposures within the home—it rests with our new Consumer Product Safety Commission. As part of their responsibility they can control the performance specifications of such things as space heaters in our home; whereas if these are poorly designed or maladjusted, action can be taken to limit their distribution, remove them from the homes, etc. This has been a major problem in many dwelling places, particularly temporary dwellings such as trailers, campers, etc., where portable heaters release excessive amounts of carbon monoxide. The technique of controlling the hardware or products entering the home has a potential of assuring a minimum of pollution within the home. But it is not an easy task just to say that in your home or in my home the level of ozone or the level of oxides of nitrogen shall not exceed so much, because this involves a matter of invasion of privacy for one thing. It is a very difficult situation, and as Dr. Newill has indicated it is very hard to come by that approach in terms of the dollars and cents. A terrific big brother type of bureaucracy would have to be invoked in order to police everyone's home. But we can protect the public from unexpected and unforeseen insults on the air within our homes through the mechanism of Consumer Product Safety Commission.

Q—May I direct a question to Prof. Box? The speaker prior to you had suggested that there should be some scientific input into these congressional regulations, and you applauded that. Right now there is a certain amount of social experimentation going on, particularly attempts with negative income tax. Would you be willing to support, or can you think of an adequate kind of experimental design that one might use to propose regulations in segments of the community?

DR. BOX—Well, when the previous question was being asked I was wondering whether it was really so impossible to get information about that particular subject. It certainly isn't necessary to look into and monitor everybody's home. What we need is some kind of reasonable sample to determine the kind of level at which various dangerous substances might be present in ordinary homes. And I know in the case of testing industry's products that this kind of thing is done all the time. People go to a home with some slight incentive which might really be the fact that this is even going to do some good. People are prepared to go to a little trouble to have censuses in the home, and I would imagine that on a sample basis, say 30 homes chosen in some appropriate random manner, this would be a great deal better than nothing to give us some idea. I think that we need to educate. Part of such an educational effort [make that part of this] would emphasize that the public can do something—it can volunteer to be part of an experiment. I believe that a lot of people are around who realize that the situation is pretty desperate. They would be glad to do that.

Carcinogens—Safe Doses? Opening Remarks

Beatrice S. Orleans

General Chairperson

We are just about on time, I think. In case you are surprised that I am up here, I had to take advantage of the offer to speak given to me this morning. There was a slight oversight, and this is a perfectly wonderful time to make things right. So I am really here to introduce your introducer, who is Dr. Nancy Mann. My reason for doing this is because this morning Dr. Wands kept saying that I was the spark plug for this Symposium. I felt that you should all know who the spark plug to the spark plug was—it is Dr. Mann. She started two years ago. This is now the third conference which has the name *Statistics and the Environment*. This was a germ in her mind two years ago which started that first conference in California. The second one was also in California. So my spark plugging started then, and it took these two years for me to find the interest and perhaps get the wherewithall and the knowledge to start something in Washington. With these introductory words I turn this meeting over to Dr. Mann.

Carcinogens—Safe Doses? Introduction

Nancy R. Mann

Senior Scientist, Reliability and Statistics, Advanced Programs, Rocketdyne, Rockwell International, 6633 Canoga Ave., Canoga Park, Calif. 91304

Thank you, Bea. I guess Bea has already said what I was planning to say concerning the history of Statistics and the Environment, that the first two of these symposia were held in Southern California. What she didn't say was that they both involved the hard work and cooperation of many people from the Southern California Chapter of the American Statistical Association and from other professional organizations in Southern California.

This present Symposium, I believe, stresses more of the health aspects and fewer of the other aspects of environmental problems than did the West Coast meetings, and the title has been changed from "Statistics and the Environment—A Symposium on the Application of Statistical Techniques to the Analysis of Environmental Problems" to "Statistics and the Environment—a Forum for Interdisciplinary Interaction." The spirit of what was originally

intended appears, however, to have remained intact.

I would like now to introduce the first speaker of this session, Dr. David Platt Rall. Dr. Rall is the Director of the National Institute of Environmental Health Sciences. He has been in that position since 1971. Since 1971 he has also been Assistant Surgeon General of the U. S. Public Health Service. Dr. Rall holds both an M.D. and a Ph.D. in Pharmacology from Northwestern University. Currently he is U. S. Coordinator, Environmental Health Program, U. S.-USSR Health Exchange Agreement and a member of the Editorial Board, Pharmacological Reviews. He is also a member of the Graduate Council of the George Washington University. Dr. Rall has authored over 100 published papers relating to comparative pharmacology, cancer chemotherapy, blood-brain barrier, blood CSF barrier, pesticide toxicology, and drug research and regulation.

Problems of Low Doses of Carcinogens

David P. Rall, M.D., Ph.D.

National Institute of Environmental Health Sciences, P.O. Box 12233, Research Triangle Park, No. Car. 27709

My assigned topic today is the question of how to assess for carcinogenic potential those chemicals that we find in our environment. It is, I suspect, unnecessary to dwell on the problem of cancer as a serious public health threat. There have been estimates suggesting that as much as 80% of the cancer in man in the United States is related to environmental chemical factors. It becomes really of enormous importance to eliminate as much as possible, as much as feasible, carcinogenic compounds from the environment. This area of discussion has in the past, and I am sure will in the future, often generate rather more heat than light, particularly with respect to the role of animal testing in environmental carcinogenesis. The classic statement is that the proper study of mankind is man. I think there is an undercurrent of feeling amongst some people that perhaps the use of animals studied appropriately or inappropriately in carcinogenicity testing is not as necessary as it is claimed to be. It seems to me that we must in fact use animal tests, today at least, as the basis for prediction of carcinogenic activity. Surveillance of the human population or selected subsets of the population for incidents of tumors is very, very important; but this is a last resort. If, in fact, an agent does enter the environment that does cause cancer in man, by the time it would be detectable in any sort of reasonable disease surveillance system, we would already have a massive epidemic of environmentally caused carcinogenesis. It is too late a point in time to have identified the carcinogen. Secondly, the manpower resources in the United States in terms of chronic epidemiology are so woefully weak in numbers—not in quality, but in numbers—that it would simply be unrealistic to view, any time in the near future, epidemiology taking on any more than it is doing right now. This is a matter of fact, an urgent national problem, that I hope can be addressed as soon as possible. We simply do not have enough capability in chronic epide-

miology, and we must begin to get more. Finally, the view is that if cancer is proven in man, and everybody agrees that the compounds are carcinogenic in man, this would tend to end controversy. I think history would prove that this is not true. Some of you may be familiar with the University Group Diabetes Program, which seemed to an innocent non-epidemiologist like myself a reasonably designed and executed prospective study with a rather straightforward outcome. It is inconceivable to believe that anything involved in carcinogenesis would generate less controversy than that study evolved. Therefore we are stuck with animal studies.

I would like to spend my time describing the problems of using animal studies to extrapolate to man. I shall concentrate more on problems of comparative pharmacology, physiology, and toxicology and leave the statistics to Marvin Schneiderman. (On the other hand, he isn't going to talk about statistics either.)

Fig. 1 presents a way of looking at this

I. Median mouse vs. median man

II. Genetic and environmental heterogeneity in man

Fig. 1. Assessment of environmental chemicals for carcinogenesis.

which I shall try to develop—that is, trying to take results from a well-conducted animal study and applying them to man. There is, first, the systematic differences between the species that you are looking at in the laboratory, the mouse, and the species that you are trying to extrapolate to, in this instance, man. I would like to divide this up into first a "median mouse" to "median man" consideration. That is, in a very homogeneous population under strict environmental control, what are the differences in response between a very small mammal with its own peculiar set of metabolic processes and a relatively large mammal, a man with his own peculiar set of physiological, biochemical, and pharmacological processes? This is the first step. The second step then is to look at the final organism we are trying to protect; that is, one individual person in a very large population, a very diverse population within the United States. Here we must get into the genetic and environmental heterogeneity in man.

To make discussion smoother I would like to present this in a somewhat different organizational rubric where I would like first to consider some differences and sensitivity in laboratory animals with respect to pharmacologic receptor differences, temporal, and size differences; then discuss some problems of population difference; and then very briefly some problems of environmental differences.

Fig. 2 shows some problems of

I. Sensitivity of laboratory animals as compared to man
   A. Pharmacological differences
   B. Receptor differences
   C. Temporal differences
   D. Size differences

II. Population differences
   A. Size
   B. Heterogeneity
   C. Selected nature of test population

III. Environmental differences
   A. Nutritional
   B. Physical
   C. Chemical

Fig. 2. Assessment of environmental chemicals for carcinogenesis—differences between test animals and man.

pharmacological differences between one species and another. We must recognize that before a compound acts at its final site of action, whether this be a compound interacting with DNA in a bone marrow cell to initiate a leukemia or what, there are a variety of steps that compound must pass through before it reaches this final site of action in its final chemical form. Each of these steps from absorption and distribution to metabolism and excretion and finally its arrival past some variety of cell barriers and its ultimate interaction with that final receptor enzyme or chemical can vary from one animal species to another. Some vary in a predictable way. Briefly, it is rather well known that absorptive mechanisms are not terribly different between various species. One interesting problem is the different hydro-

gen ion concentrations in the stomach of some of the herbivorous and carnivorous animals. I shall come back to this problem of distribution later because this seems to be more a function of size than a species difference. Now metabolism—the xenobiotic metabolism of foreign compounds—differs greatly from species to species. Some recent work is beginning to suggest some general principles in the differences which may be of importance. It is quite clear that herbivores in general have a much more active xenobiotic metabolism system than carnivores. This was perhaps first brought to our attention when the veterinarians in the zoo tried to anesthetize a tiger with pentobarbitol (which works very well with small mammals). The tiger fell asleep promptly but never woke. Since this was a prized animal, comparative pharmacology became quite important. The metabolic patterns are increasingly important because we are beginning to realize more and more clearly that very often the compound that was administered is not the ultimate carcinogen, and it takes metabolic processes within the body to create the active compound. There are some differences in excretory rates between species but these do not seem to be of major importance. The various cellular and intracellular barriers seem to be surprisingly constant throughout the vertebrate kingdom. With regard to receptor differences and the ultimate mode of action, it seems that this is surprisingly constant in the vertebrate kingdom. A molecule of DNA from a mouse, a rat, or from a man is not very different, and the interactions of that molecule with chemicals which come ultimately from the environment are surprisingly similar. However, there are temporal differences which I think have not been considered in the past. It takes time to develop a tumor, and at least some of that time is related to the actual cell division process. The renewal rate of the bone marrow or of the gastro-intestinal tract of the mouse can be compared with the rate in man. The cell division rate is significantly faster in small animals. The cell cycle times are about half, the cell turnover periods are about double in man. Mice and rat cells turn over faster. The latent period for the development of tumors is faster in mice and rats. One example of this is shown in Fig. 3, the latent period for the development of thyroid tumors after radiation iodine administration in the rat, the dog, and man. Rats developed the tumors in the order of 1–1½ years, dog with a spread from 4–10 years, and man took close to 12 years to develop the tumor. There is apparently a systematic difference in the latent period related to body size.

It is important also to realize that the life span of man is about 35 times that of mouse or rat. How can we put this all together? The cell division time is twice as fast in the smaller animal, so there is in a sense twice as great a chance for some untoward event to happen. The more rapid cell division rate in part must account for the shorter latent period in the very much smaller animals. However, the life span of man is so much longer indeed that there is much longer lifetime opportunity to develop a tumor. I would suggest that what I have run through is a very simplistic view of these temporal differences. But I think in the future we should spend more time considering them as we consider the implications of

Absorption

Distribution

Metabolism

Excretion

Arrival at site of action

Ultimate action

Fig. 3. Steps a drug must pass before it can act.

lifetime studies in small animals for lifetime exposure in large animals. I think as we learn more about the actual mechanism of carcinogenesis in experimental studies, this view of the temporal differences between very small and very large animals might be very useful indeed.

Now let us move into problems of size differences. The size determines in many ways the rate of distribution of foreign compounds throughout the body. To take a very simple example, the blood volume of a mouse is about 1 ml. The cardiac output of that mouse is about 1 ml./m. The mouse turns over its blood volume in about 1 minute. In man the cardiac output is only about 1/20 of the blood volume in man. The mouse moves things around about 20 times faster than man. Thus, the exposure of a tissue to a compound in a small animal occurs more rapidly. But excretion also would be much more rapid, and on a weight basis small animals excrete compounds more rapidly. Therefore, it is reasonable to expect that small animals would be able to tolerate larger doses of compounds. Fig. 4 shows the toxic doses of a number of anti-cancer drugs, to compare, not on a weight basis, but on either a surface area or a weight to two thirds power basis. There is reason-

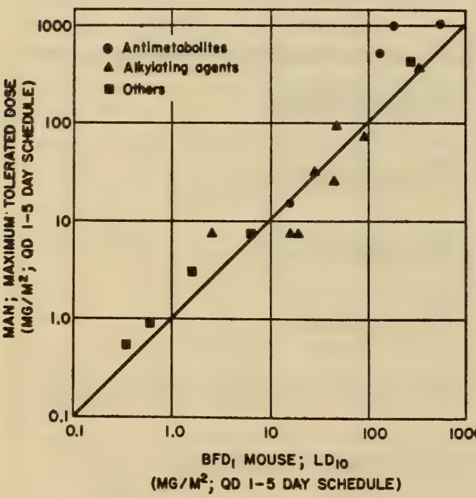

Fig. 4. Toxic doses of a number of anti-cancer drugs.

ably good agreement between the human toxic dose and the BFD_1 mouse toxic dose. In essence man is about 12 times more resistant than the mouse. There is another aspect to this slower rate of distribution and metabolism in the large animals that I think is important, and that is in terms of long-term studies. Fig. 5

Average USA Man and Average Mouse on 2 ppm in Diet

Intake	Mouse	10 ug/day	3.6 mg/year
	Man	30 ug/day	10.8 mg/year
Total Intake	Mouse	1-2 years	5 ± mg or 200 mg/kg
	Man	20-30 years	250 ± mg or 4 mg/kg

DDT Concentrations in fat = 5-6 ppm in man and mouse.

Fig. 5. DDT intake in mice and man.

presents a mixture of data from the U. S. Market Basket Survey, from the Pesticide Survey on the human levels of DDT, and from an IARC (Lyon) report on the fat and tissue levels of DDT in a carcinogenesis experiment in mice. The intake of the mouse was 2 ppm DDT in the diet. This was about 6–8 μg/day, or about 3 mg/year. Man, according to the Market Basket Survey, 3 or 4 years ago ingested about 30 μg/day of DDT or a total of about 10.8 mg/year. The total intake in the mouse over 1 to 2 years of the experiment was about 5 mg, or a total of 100 mg/kilo. The average DDT concentration in the fat of the mice at sacrifice at the end of the experiment was 5 to 6 ppm. Man in his 20 to 30 years' exposure to DDT had a quarter of a gram or about 4 mg/kgm total exposure; but this steady state fat concentration on the average was about the same or about 5 to 6 ppm in the fat. We need to know more about the final concentration of the compound in the experimental animal and the exposed human population.

There is one other aspect to the size difference which I would like to touch on very briefly. The large animal has a very much larger number of susceptible cells in his body that may interact with the potential carcinogenic agent. For instance, there are from 160 to 2000 times more susceptible cells in one man than in

one mouse. Thus, one man is equivalent to at least a 160-mouse experiment. If there is a relationship between the initiation of a carcinogenic event and the number of susceptible cells, and this to me is logical, then one man is possibly more sensitive than one mouse.

Let us now move on to population differences. The first problem, one that has been extensively discussed, concerns the problem of extrapolating toxicity or carcinogenicity results from a few hundred laboratory animals to a few hundred million people. Another major problem is the heterogeneity of the human population. I believe Fig. 6 illustrates this very well. What is shown is the steady state plasma level of a tricyclic antidepressant given to a number of patients at the standard clinical dose after allowing a steady state to develop. In this random group of patients the plasma concentrations at steady state varied from about 10 μg/l to 300 μg/l in plasma, an enormous variability. So it is pertinent to ask, if one is trying to extrapolate data from a laboratory experiment to man, does the laboratory experiment reflect those patients on the far left corner, the middle, or the right corner? There can be very great differences. I have shown this only for the metabolism of this one drug. The body rids itself of foreign organic compounds largely by metabolic rather than purely excretory mechanisms. This is largely a difference of xenobiotic metabolic pathways, yet every aspect of the handling of a compound by the body is potentially involved in such human heterogeneity.

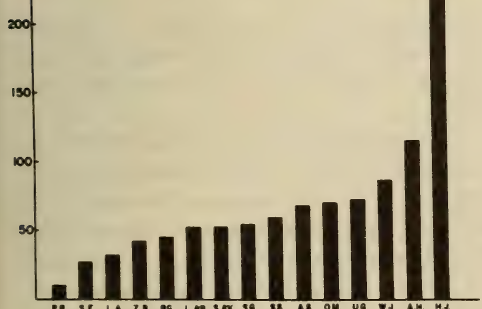

Fig. 6. Steady state levels of NT during daily oral dosage of 3 × 25 mg.

It is also necessary to consider the very selective nature of the test population. Laboratory scientists go to all ends to select vigorous, well fed, healthy animals to extrapolate to a population which contains sub-populations that have all varieties of illness, weakness, and disease. Thus, population differences related to size, to genetic heterogeneity, and to the very selected nature of the test population are important.

Finally, there are environmental differences which I shall touch on briefly. I think many of these are obvious. Nutritional differences clearly can alter response to carcinogenic agents. This is well documented. The laboratory animal is on a diet that is well supplemented with vitamins, minerals, adequate proteins, and so forth, while many segments of the American population have diets of varying quality. The possibility of significant differences is apparent there. The physical environment—heat, light, ionizing radiation, etc.—can affect responses. Again we know the very great difference between a well controlled animal room and the human situation. Perhaps the major problem is the chemical environment. The proper laboratory scientist makes every effort to be sure there are no mycotoxins in the feed for his animals and that there are no nitrosamines in the feed for his animals; the next morning he sits down and has bacon for breakfast. With the various potentially toxic compounds in air and water and food and with concurrent drug administration there exists a great opportunity for synergistic toxicity. This is a problem that is only beginning to be approached in the environmental field. In the field of therapeutic drugs, the joint toxicity of two drugs has been demonstrated many times.

These differences, nutritional, physical, chemical, and environmental, all must be considered in any attempt to use laboratory animal toxicity or carcino-

genicity data to extrapolate to man. The net result of all of these differences suggests to me at least that the laboratory animal is not a sensitive indicator of carcinogenicity in tests with environmental chemicals. If results from laboratory animal tests are to be used to set up guidelines to protect very large human populations it is prudent to be extremely conservative in trying to apply this extrapolation.

Another way of looking at this is shown in Fig. 7. Some of you may have read an article in *Science* about seven years ago about some behavioral scientists who had been studying LSD in the cat and wished to see what happened in the elephant. They gave the mg/k dose of LSD which provided whatever behavioral response they wanted in the cat to an elephant borrowed from one of the local zoos. The result in a relatively few minutes was a very, very large elephant convulsing, defecating, and finally dying. What I would like to suggest is that we must not forget this principle of comparative pharmacology and toxicology as we try to extrapolate data from laboratory animals to man, or we may be associated with a very large convulsing and defecating elephant.

Fig. 7. "I just got tired of rats and mice, rats and mice."

Safe Dose? Problem of the Statistician in the World of Trans-Science

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When the statistician works on an issue in the public arena he often finds that the data he collects, and the manner in which he analyzes the data are conditioned by things outside his own professional competence. This paper gives some examples that attempt to discuss what the statistician might do that despite these pressures he might provide, if not an unbiassed picture, at least a fuller picture. Because I am from the National Cancer Institute, I am mainly concerned with the problems of what causes cancer, how we determine that a material is a carcinogen, and the statistician's role in establishing "safe" doses, if there are such things.

The statistician is constrained by the biological models of his laboratory colleagues. If the research worker with whom you are working is of the opinion that there is threshold in carcinogenesis, i.e. there are some doses that are sufficiently low so that they will not produce any cancer whatever, then it is extremely likely that he will design experiments

(consciously or unconsciously) that will yield data that point to the existence of a threshold. If on the other hand, if the biologist with whom you are working is a man who questions the threshold concept, his data likely will be developed in such a way as to demonstrate that the probability of a threshold is either extremely unlikely or data are of such a nature that you can't demonstrate whether a threshold exists or not.

If there are difficulties in unravelling threshold in the laboratory, the difficulties are multiplied many fold when we try to interpret the results of exposures of humans to potentially harmful materials. I will give an example from asbestos exposure. Asbestos hazards have been in the headlines recently and much work, some of it of very high quality, has been done. In reviewing the published papers in the relation between human asbestos exposure and the possible development of cancer, I found that two authors, McDonald (1972) and McDonald *et al.*, (1971) of McGill University in Canada and Enterline *et al.* (1972, 1973) of the University of Pittsburgh in the U. S., have attempted to develop a quantitative dose response relationship. McDonald and Enterline have used the same measure of exposure, millions of particles per cubic foot years (MPPCF years). A physical measure was taken of the number of particles present in a sample of air in the vicinity of the worker and then this multiplied up by the number of years that the worker was exposed at those levels. There are difficulties in such a dose measure. Workers are not at the same job all of the time, the levels of exposure are not the same all the time, and, thus, the dose for any specific worker is only an approximation. Further, there is always the problem that not all the particles measured in their millions of particles per cubic foot years are asbestos particles. Asbestos is a very difficult material to identify and measure in its submicroscopic state. Of all the papers I have read these two authors are the only ones who have attempted to quantify dose to give a dose response relationship.

There are some differences between McDonald's and Enterline's studies. McDonald's population is a population of working asbestos miners in Canada. Enterline's population is a population of retired industrial asbestos workers in the United States. McDonald measures his response in terms of equivalent average death rate. Enterline measures his response in terms of standard mortality ratio. I don't know how to equate these two. In the figures here, I attempted to put them on the same scale. Fig. 1 gives the mortality rates for cancer of the bronchus and lung for McDonald's measure of equivalent average death rate and for Enterline's measure standard mortality ratio. I have equated equivalent average death rate of 10 with a standard mortality ratio of 100. This is very likely to be wrong. I don't know what to equate in the equivalent average death rate to standard mortality ratio. In Fig. 1 the standard mortality ratio is 10 times the equivalent average death rate.

Fig. 1 shows two dose response curves of roughly the same shape. The solid line is fitted to the solid dots; those are the McDonald data. The dashed line is fitted to the x's, the Enterline data. In one paper, Enterline combined the three doses under 125 mppcf years, into one single dose group and that is shown on the figure by an x in a circle.

Because of the problem of equating equivalent average death rate to standard

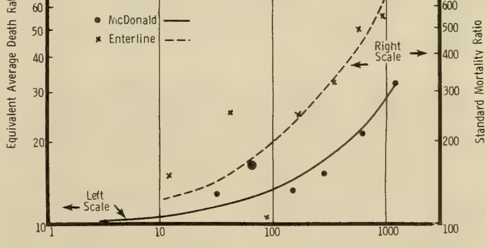

Fig. 1.—Mortality rates for cancer of bronchus and lung.

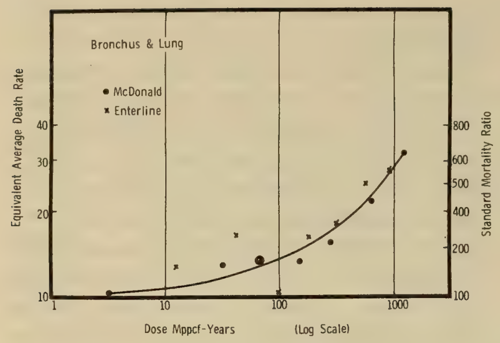

Fig. 1A.—Mortality rates for cancer of bronchus and lung, with scale changes (see text for details).

mortality ratio and because these two dose response lines don't seem to lie together, I have modified Fig. 1 into Fig. 1A. Here I have squeezed the standard mortality ratio scale down by a factor of two. I have taken the equivalent average death rate of 20 to equal a standard mortality ratio of 400, equivalent average death rate of 40 to equal a standard mortality ratio of 800 and so on. With this scale change on Fig. 1A, it looks as if a single response curve might be fitted to all the data. The McDonald and the Enterline data now don't seem far apart. As I have drawn this figure it looks as if there could possibly be a threshold in the vicinity of dose of under 10 mppcf years, although this is really quite uncertain. Concerning the possibility of threshold, McDonald says "The excess was virtually confined to persons with a dust index of over 200 mppcf years." Enterline says "There appears to be no direct relation between dust exposure and respiratory cancer below 125 mppcf years. Important increments in respiratory cancer mortality apparently occurred somewhere between 100 and 200 mppcf years."

It is difficult for me to talk about excess with respect to McDonald's data because his measure of the equivalent average death rate essentially has no "normal" against which to measure excess. Enterline's standard mortality ratio measure does give an opportunity to measure excess and I find it interesting that all his points below a dose of 100 lie above the standard mortality ratio of 100. These all indicate an excess mortality. There is no question that Enterline's statement that there is no direct relationship between exposure and respiratory cancer below 125 mppcf years is correct. Should the data above 125 mppcf have any effect on what one says about what happens below 125 mppcf? At least two interpretations are possible of these sets of data. One: there is a threshold (although it is chancy). Two: there is no threshold shown.

To examine the threshold concept a little further, I have reproduced Enterline's data in a table. Table 1 shows the dose, the standard mortality ratio at this dose and the 95% confidence limits on the standard mortality ratio. I have both combined the three lowest doses as Enterline has done and also presented the 3 lowest doses separately. We have equivocal results. With the 3 lowest doses combined there is a standard mortality ratio of 166.7; the confidence limits on this standard mortality ratio range from 93 to 275. Since 93 to 275 includes 100, one can say that these lowest doses are *not* different from 100. On the other hand, with an upper confidence as high as 275, the data are consistent with a substantial effect.

Have we demonstrated no excess for these three lower doses or have we only shown problems concerning the small number of persons exposed at the three lower doses? Was follow-up as good for the short-term workers (who then got low

TABLE I

Dose MPPCF Years	Standard Mortality Ratio		95% Confidence Limits on SMR (Haensel, 1962)	
<25	166.7	153.8	93–275	18–555
25– 62.4		258.1		112–509
62.5–124.9		108.7		35–253
125–249.9		250.0		129–437
250–499.9[a]		326.9		lower limit
500–749.9		500.0		well over
>750		555.6		100

[a] This is given as 400 by Enterline, but that appears to be a misprint.

doses) as for the longer term workers who got the higher doses? Should the people in the regulatory agencies be suspicious of a result that has such a high upper confidence limit or should they say that since no significant excess has been demonstrated, that a safe level has been demonstrated?

The data given so far are concerned with problems of the inhalation of asbestos. There is more current concern over ingestion from water, or food. We would like to find out what happens when asbestos gets into the digestive system. On Fig. 2 are the data from McDonald

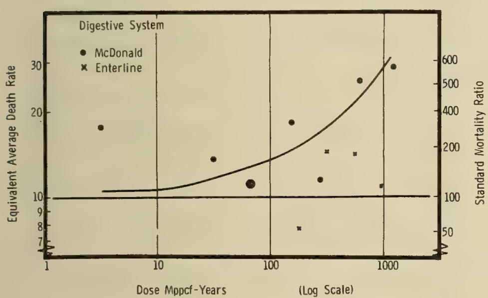

Fig. 2.—Digestive system cancers vs. exposure.

and Enterline showing the digestive system cancers vs. exposure. There were far fewer digestive system cancers reported then bronchus and lung cancers in this group of workers. The data show a wider range of fluctuation. In the McDonald data there are at least 2 inversions. Yet at his lowest dose level, somewhere between 1 and 10 mppcf years, he has an equivalent average death rate well above 10. The Enterline data also show some inversion. I plotted Enterline's 3 lowest points as he has done into a single point, an x with a circle around it. The next highest dose shows a standard mortality ratio of under 100. If there were a threshold fluctuation in sampling would give some rates below 100. The next two doses show SMR's over 100, and the next dose shows a lower standard mortality ratio, an inversion.

What could one say from these data? The McDonald data seem to say that inhaled asbestos, which then gets into the digestive system, is quite likely a digestive system carcinogen. The Enterline data lead to no such clean conclusion. To try to make more sense of these data I have combined some of the dose groups within each set. It seems to me that there is nothing sacrosanct about one particular dose range as compared to another, hence my dose groups are as "valid" as any.[1] The effect of combining various dose groups is shown on Figure 2A. The

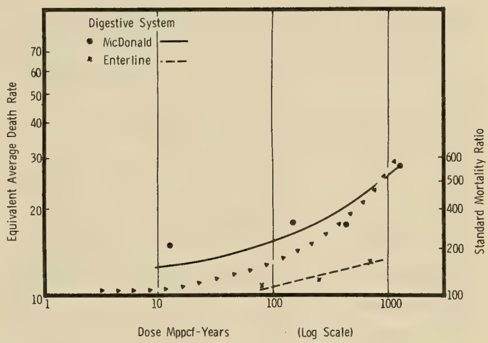

Fig. 2A.—Digestive system cancers vs. exposure, showing effect of combining various dose groups (see text for details).

dose response curve that was on Figure 2 is now shown on Figure 2A as the line made up of small triangles. The McDonald points, which in Figure 2 comprised six dose groups, have been collapsed into four. Enterline's data which comprised five dose groups have now been collapsed into three. The McDonald data now show a distinct dose response relationship lying well above the equivalent average death rate of 10 which I have previously suggested was "normal." The Enterline data show a dose response curve below, but perhaps paral-

[1] This is not strictly true. William Cochran (1968) discussed this problem in a Rietz Lecture published in 1968 in *BIOMETRICS*. My colleague, John Gart, has given me some references (Connor, 1972; Gart, 1971; Hamilton, 1974) showing how to compute a dose-response relationship for data like these without combining data into groups. One needs to have the individual data, of course. I will suggest Gart's approach to both McDonald and Enterline.

lel to the McDonald's, but still lying above the standard mortality ratio of 100. Are these data consistent with a threshold? McDonald's data are not consistent with a threshold. The Enterline data could be. If one continues the straight line that I have drawn for the Enterline data, it would come down to or cross the standard mortality ratio of 100 line somewhere between 10 and 100 mppcf years.

What have I shown here? Even a set of rather well collected data can be looked at in several different ways. The different ways might lead one to exactly opposite conclusions with respect to the important question of whether some human data have or have not demonstrated the existence of a threshold. Since when one is concerned with establishing the existence of the "safe" dose, one must be able to establish the existence of a threshold, it then seems to me that the statistical problem of establishing a "safe" dose becomes effectively an unsolvable problem. This puts us into the field of trans-science in the Alvin Weinberg sense (Weinberg, 1972). There is not much that we can do within science to answer that particular question. We must go on and look at some other ways to handle and solve this issue of so-called "safe" doses.

As statisticians we try to "model" the real world. The statistician in looking at a dose response relationship often finds that he is working with one of several mathematical models—in biology, usually one of three models. The probit model makes the assumption that the response is linear (as the integrated normal curve) against the logarithm of the dose. The second model is generally the logit model and it derives in part from certain kinetic considerations. The third model is the so-called "one-hit" model. This model postulates that one event is all that is necessary to create an activity and that this activity leads to an observable response. In the most extensive studies of radiation as a carcinogenic process, research workers and the technical reviewers seem to have come to an uneasy agreement that the one-hit model represents what is going on there. The appropriateness of the model becomes an issue that is not solvable by the statistician alone. Whatever model the statistician uses for his dose response model must have reality in the biology. And the statistician is not the judge of what is the reality in the biology, though his opinions are valid. He has to pay attention to those people who say that it takes some minimum number of molecules, not a single molecule to produce an effect. He has to pay attention to those theorists who would define cancer as a irreversible, self-replicating change. That is, once an event occurs it causes a change, perhaps a change in the genetic material of the cell, and this is self-perpetuating. That's very close to a one-hit concept.

However, no matter what the reality of the biology, these three major mathematical models of the biological dose response give, in the real experimental world nearly identical results for most of the dose range. In the range in which most work is done and given the size of most experiments, these models are indistinguishable. In a paper by an advisory committee of the Food and Drug Administration (FDA, 1971), the three models were compared over a 256-fold dose range over which they were nearly identical. Differences occur when one tries to extrapolate to the very low doses. In general the probit model has the highest order of contact; the one-hit model the lowest order of contact. The probit model having the highest order of contact, it says that the dose that it takes to produce a 1 in 1 million effect is higher than the dose that the logit or one-hit model would call for. Therefore, the model that one chooses is of considerable consequence when one wants to talk about responses at very low doses. And, of course, in the environment to which we are exposed, for most people we are concerned about very low doses. It does not seem to me at this time that it is possible for us to choose among these three models down at the very low doses. In fact, as a statistician wandering in biology, I am convinced that none of these models is ap-

propriate at very low doses. Certainly not for the heterogenous human population.

Whichever of these models one chooses, one is usually working in a yes-no situation. The statistician analyzes data after someone else has decided that there is a tumor or there is not a tumor. The pathologist says an animal has a tumor or the animal does not have a tumor. It is to this kind of situation that these three common mathematical models pertain. Usually there is more information in an experiment, i.e., the time-to-appearance of the tumors. Early work by Druckrey (1967) of Germany related the time-to-appearance of tumors to the dose of the carcinogen. The larger the dose the earlier the tumor and the shorter the so-called "latent" period. If we could take advantage of this kind of information, perhaps we could demonstrate that with very low doses the tumors might be expected to appear so very late in a lifetime as to be of no consequence. Albert and Altschuler (1973), starting with the Druckrey concept have attempted to produce a time-to-appearance model carcinogenesis. Their work was published in the Proceedings of a Hanford Symposium and generally is not easily available. This is unfortunate because more people should exploit this model to see what it implies. In its present stage of development the model has some flaws. Their time distribution for the appearance of tumors has been questioned. They use the lognormal distribution and several people (Pike, 1966; Peto *et al.,* 1972) disagree that this is the appropriate distribution. Gehan showed that it has a peculiar hazard function (Gehan, 1969). Albert and Altschuler considered the problem of the median time-to-appearance of tumors. This is inappropriate if we want to extrapolate to man. What we want to know is the time-to-appearance of the tumors in some very small per cent of the population, i.e. 1/10% or 1/1000%, etc. Finally, theirs is an estimating, or extrapolation model, and we need a way to put in a "guarantee" that the risk shall not exceed a certain amount. Mitchell Gail (1974) of the National Cancer Institute looking at the data on the study of the United States veterans with respect to lung cancer and smoking found lung cancer, if it were the only cause of death (as in an Albert/Altschuler computation) would have a mean time of appearance of about 320 years. But lung cancer is a serious problem because a good deal of lung cancer appears long before the age of 320.

The Albert/Altschuler work considers the life shortening effect of cancer not just the appearance or non-appearance of cancer. Since everyone must die at some time, the fact that a dose of a material produces a given number of additional cancers is not of as much consequence as if it produced that many cancers (or even fewer) at young ages. David Hoel and colleageues (1972) have done some work on this problem. Mitchell Gail attempts to estimate what he calls "three measures of merit." His first measure of merit is the actual life shortening that would occur in the whole population— given a new form of cancer or given that cancers appeared at some given age in the population and in some proportion of the population. Dublin and Lotka many years ago showed that all of the cancers in the population in the United States would reduce average life span somewhat of the order of less than 2 years. The second measure of merit is the life shortening for those people who develop cancer. For these people the shortening is a good deal greater. It ranges from 12 to 15 years more or less depending on the nature of the cancer. Finally, Gail (1974) adds another measure of merit. This measure of merit is the one of directly asking what does the cancer cost by asking how much life shortening it produces before some given age. Murray and Axtell (1974) of the National Cancer Institute have looked at the "costs" to the United States economy for all the cases of cancer who died in one year. They found that by taking the average life span and finding how much of the working life has been lost by cancer victims and multiplying this by the average annual income for persons employed at that time, that one

year's death from cancer in the United States cost of the order of $18 billion dollars. They do not include medical costs, or any secondary costs to the families.

The problem of cost is not simple. The British Department of Welfare and Social Security (1972) asked the question, "Suppose we were able to reduce smoking in current British smokers by 20% or 40%, what would the net monetary effect be?" Up to sometime in the 1980's or 1990's there would be a net gain to the British economy, but following that there would be a net loss. The net loss would occur because those persons who had not died from their smoking-related diseases would live long enough to draw pensions and the costs of the pensions would exceed the contributions (monetary) that these persons would have made to society by extending their working lives. This particular example shows the problems of a quick look at a cost-benefit analysis. As persons get older in our population they are no longer producers and they cost something to our working population to keep them alive. A simplistic cost-benefit analysis taking this into account might consider that these persons were not of any particular worth. A logical conclusion from such a cost benefit analysis would say that these people are costing us more than the society is benefitting from them. Therefore, there is no good reason to keep them in the population at all. One wonders at this time whether one should take an Orwellian point of view and by carrying this cost-benefit analysis to its somewhat silly, logical extreme and see to it that people did not smoke but also to see to it that they died promptly at the age of 65 so that they would not draw any pensions. I'm not recommending this.

There has been substantial talk and little work done on the problems of cost-benefit with respect to materials that may be carcinogens that are added to our environment. There could be important gains from some of the food additives or some of the pesticides like DDT. Since these materials have great economic importance, an attempt has been made to equate the economic gains from increased food production following from using a pesticide, to the economic losses associated with illness and premature death from cancer. With respect to the cost-benefit computations, I think first, the "logical" results from the British Department of Welfare and Social Security should be kept in front of our faces. Second, the answers to Cornfield's question need to be considered openly. Cornfield asks the question "costs to whom and benefits to whom?" Are the costs and benefits to the same person? If the costs and benefits are not to the same person what action then is appropriate for societies to take? Is it appropriate for me to benefit by having my electric bill reduced at the expense of some workers in atomic energy electrical plants getting too high a dose of radiation and dying sooner?

There are many situations in which benefits might accrue to only one portion of the population and the costs to another portion of the population. What are society's responsibilities within this set of circumstances? Is society as a whole responsible since the benefits accrue to Society (now with a capital S)? Is Society responsible for ameliorating the costs? Does it mean that Society should pension off the family of the wage earner who had died early of a bladder cancer as a result of exposure to an industrial carcinogen which is used in making dyes from which all of us benefit by having the more brightly colored environment about us? Should this worker's family get a full pension equivalent to his income for the remainder of his working life that he may have lost? On a much lower scale, should all this worker's medical costs be born by "Society"? I put Society in capitals here because we must ask who makes up society? Is it the Federal Government, the local government, the community? We all pay for what some of us gain.

Finally, with respect to the cost-benefit problem in general, let me give a not-so-hypothetical example. Let us say that the American Cancer Society in its efforts to cut down deaths following smoking

develops an effective program in anti-smoking propaganda. Let us say that their program is effective in reaching young people and those other groups in the population that their older programs have not been so effective in reaching—the Blacks, the women, etc. This is a program that will have some economic costs—you and I contribute to the American Cancer Society and that's a cost. It will have some benefits. There will be people who will live longer, who will not die of lung cancer or perhaps cancer of the bladder, or one of the other things associated with smoking. Although the American Cancer Society is doing it, some of these people will not die of heart disease or emphysema or certain forms of bronchitis. And finally, some of the people will live long enough to get on the pension roles and remain there for a long time. To whom should these particular costs now be ascribed? On whose account do we check out that these things are costs? If the American Cancer Society's program is very successful it may be that there will be some industrial workers who are put out of jobs—people manufacturing cigarettes. It may be that the tobacco farmers will not be able to grow tobacco and not get that income. If they move from tobacco to say soybeans and get a higher income, should that be reckoned as a negative cost? I don't know the answer to any of these questions, but it seems to me that the cost-benefit problem is a very much more complicated one than we have realized in the past. It also seems to me that the problem of computing costs and benefits can not be left to people who are interested parties. I don't know in whose hands the computations ought to be put, but just as my first example on the problems of how one interprets the asbestos data indicated that the same set of data might very well be interpreted differently by different people, it also seems to me that the computations of costs and benefits will come out to be very different if done by different people. I'm not asking that statisticians be appointed (or anointed) to do these computations. Statisticians are no more free of their personal cultural histories than anyone else. Michael Polanyi (1969) pointed out many years ago that the scientific ideal of an absolute truth divorced from human judgment is worse than foolish—it also impedes scientific progress.

Thus, it seems to me that the problems of determination of "safe" dose are problems that transcend our field of statistics. They are problems that transcend the field of the laboratory worker; they are problems that transcend the field of the epidemiologist; they do not seem to me to be problems that can be solved even by those of us in the three fields working together. The problems of social costs which flow from our determination of "safe" doses require a whole group of other kinds of input. What then can be done to attempt to help assure that we have a safer society within which to live? I'd like to give two sets of recommendations—one from a colleague who has worked and thought about this problem at some length and one from a well-known geneticist. Here's what my first colleague says: "Do monitoring. Use registries and record linkage to detect sudden increases in space-time occurrence of the kinds of diseases we are concerned with. When followback reveals that these are due to some specific drug or chemical we are already in a bad situation. A great many people have already been exposed but at least the causative agent can be recognized and if it is then removed from the environment perhaps we can prevent an epidemic." The implementation of his suggestion requires that there be alert people, groups of experts looking all the time for these sudden increases or clusters. Many of the things reported as sudden increases or clusters will turn out to be dead ends, useless leads. This is comparable to the occasional breaking through the limits in a quality control chart and where we find nothing wrong, no departure in our process. Nonetheless, these unusual events will still have to be examined. They will have to be

investigated just as we do in quality control.

What about things that are not yet in the environment? We certainly must do animal testing. We must screen materials for carcinogenic effects in rodents and perhaps in higher animals. In spite of all the difficulties that Dr. Rall expressed in the last paper, we must pay substantial attention to these results. Materials that appear to be carcinogenic in these experiments will probably have to be excluded from the environment. Some exceptions probably will be made or can be made for drugs or related materials that are used to treat uniformly or rapidly fatal illnesses in which quite obviously the benefit to be gained by the person taking the drug will be very much greater than the cost of possible cancer to this person some years in the future. However, for materials in which the gain and benefits do not accrue to the same person it is likely that these materials will not be marketable. If it appears, however, that the material is of very great economic potential then obviously work on metabolism and biochemistry will have to be done. If it can be demonstrated that the material is metabolized substantially differently by the experimental animal than it is by man, then this would indicate thatwe must do further laboratory-animal research to find species in which metabolism is closer to that of man and do carcinogenic testing in them. If in such species we can demonstrate the identity of the metabolic pathway to man's and such species can demonstrate that the material is not a dangerous material, then obviously it becomes marketable. In addition we will have learned a great deal about the metabolism of different kinds of animal species. Finally, we obviously must encourage research into laboratory methods that will give us answers quickly as to possible positives. I think we have to develop some no-false negatives screening systems to cut down on the enormous number of materials that now seem to have to be tested in long-term life span animal feeding experiments. If quick methods can be developed that produce no-false negatives, even if the methods ask us to test five true negatives for every one positive they would introduce many economies in money and in time.

The second suggestion that should be taken quite seriously was made by James Crow (1973), the geneticist, in the publication of the National Institute of Environmental Health Sciences. Crow takes a lead from the work on radiation risks and with the so-called "allowable" increased dose of radiation permitted from sources such as the production of power through atomic energy. Crow notes that among the early maximum levels that were established by such groups as the BEIR group (1972) and others was an addition of radiation to the environment roughly equal to the amount that one naturally received from nature. Crow further suggests arithmetically converting the hazards from chemicals to a radiation equivalent dose, and setting this equal to the early "maximum" permissible addition, 170 millirems. In doing this, we soon get into problems of the appropriate dose response curve at low levels; what are the incremental cancers that occur given this particular dose of chemicals? If we can make this chemical-radiation equivalence perhaps even crudely, Crow then recommends that we treat any new material entered into the environment as a potential additional "burden." If this added burden then brings us up over the equivalent of 170 millirems, then action must be taken to bring the total burden down to its allowable level. In other words if we introduce a new chemical into the environment and it is of such economic importance that it must be introduced, there then have to be other chemicals that will come out of the environment, since the new materials plus the old materials would bring us up above the maximum permissible additional burden. This would create a situation in which the people who manufactured and marketed the old material might be required to present information to demonstrate why their material should remain in the en-

vironment rather than permitting the new material to enter into the environment. And the promoters of the new material would have to present the contrary arguments. Under these circumstances there would be some healthy competition as to what new materials might come into the environment. It might very well influence a company which already has a substantial number of materials on the market to not introduce another one because the new one would require that an old one be taken out of sale. It might be a useful thing for the Shell Comapny, manufacturers of Dieldren, and the Montrose Chemical Company of California, the manufacturers of DDT, to present arguments as to which of the two (or either of them) might better remain in the environment.

There is something Crow neglects and that we have not talked about here today. It presents serious statistical problems. Crow's limit assumes that each additional material added to the environment has a simple additive effect. There is evidence that this is likely not to be true. To see this one has only to recall the experience of the smoking asbestos worker as compared to the asbestos worker who does not smoke or the smoker who does not work in asbestos factory or the smoking uranium miner as compared to the uranium miner who does not smoke and again the smoker who is not an uranium miner (Doll, 1971). If we can get multiplicative effects of smoking and asbestos exposure or smoking and radon exposure it may very well be that some of the environmental chemicals we have give us multiplicative rather than additive effects. These things obviously will have to be tested and we will have to see what combinations break through Crow's upper equivalent of 170 millirems. These problems of testing multiple materials for their additive non-linear interactions, are once again problems for the statisticians in designing the experiment and in evaluating the data.

In all these activities that the statistician has to participate, he can not go it alone. He is involved with the epidemiologist in the monitoring. He is involved with the computer people in helping develop data linkage systems. He is involved with the laboratory worker in setting up the animal screening systems and he is involved with the administor in evaluating the effectiveness of these animal screening systems. He is involved with helping set up and evaluate the quick laboratory methods. Thus, I see the statistician intimately and deeply involved with much more than just mathematical theory for setting "safe" doses. I see a great many research problems that need to be worked on. I see that these can not be worked in a statistical vacuum. I see problems of social values intruding on the scientists and intruding on the statistician. There is no way that these can be escaped. Perhaps the best thing that the statistician can do is to declare his loyalties and his biases and let people then evaluate the work he has done. Since I advocate that statisticians be open about their biases, I owe it to this audience to be open about mine. As I reviewed the data relating asbestos to cancer in the first part of this paper, it seemed that my bias in coming to you out of the field of cancer research certainly affected what I did with the data, how I handled them, and how I interpreted them. I, therefore, declare I am employed by the National Cancer Institute, an arm of the Federal Government, a research agency, and my personal bias is strongly against cancer.

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Carcinogens–Safe Doses? Panel Discussion

Chairman: **Dr. Nancy R. Mann,** *North American Rockwell*

Panelists: **Dr. Harold M. Peck,** *Merck Institute*
Dr. David P. Rall, *National Institute of Environmental Health Sciences*
Dr. Marvin A. Schneiderman, *National Cancer Institute*
Dr. Jane Worcester, *Harvard School of Public Health*

DR. PECK—In industry, if a new chemical is shown to be carcinogenic by relatively simple tests, a decision is made on whether or not to proceed beyond this point of discovery, even before the information gets to the governmental level. One decision is to go ahead with research and development if the chemical is of potential great value, either therapeutically as a drug, or perhaps in the chemical industry. A second decision, which is perhaps the easiest, is to drop all interest in the chemical at that point. A third decision may be to try to alter the chemical structure so that the carcinogenic potential would be lost but the utility would be retained. Chemists and biologists in the pharmaceutical industry have employed the third alternative to advantage. Not infrequently it is possible to alter a chemical structure to retain the desired pharmacologic activity and diminish the toxicologic activity.

If the chemical under study proves to be a potent carcinogen, this fact is quite easily determined, and thus the appropriate decisions can be made. The real problem arises when the tests designed to demonstrate carcinogenicity produce results which are equivocal. Do the tests really show that the chemical is a carcinogen? If the chemical is a moderately potent carcinogen, again there is probably no real problem. It may take a little longer, and a few extra tests to show that it is or is not. On the other hand, it is very, very difficult to show that a chemical is not a carcinogen. As with any toxic effect, if you look long enough, hard enough, and in enough species of animals, you probably would eventually do a test which would suggest that the chemical is a potential carcinogen, even though it may not be.

This relates directly to the "noise" that Dr. Box mentioned this morning, and I think this is where the problem of communication that Dr. Schneiderman discussed is extremely important. Occasionally the statistician, using the results submitted to him by the biologist, will show that there is a significant difference between the results of a test group of animals and a control group of animals. The biologist may not believe this to be a fact on the basis of his knowledge of animal variation. The statistician and the biologist may not be able to communicate to each other the actual meaning of their test results. The communication between the statistician and the biologist has not been very good, but it is certainly improving. The biologist fails to understand the statistician and believes the statistician tries to deal with ideal situations which rarely, if ever occur, in the biological area. The statisticians occasionally fail to realize that in carcinogenic tests the animals being used have a variable incidence of spontaneous tumors which must be taken into consideration in evaluating the carcinogenic potential of an agent. This communication is extremely important and biologists and non-biologists should try to reach a mutual understanding. We had an analogous situation in our organization in which the toxicologists and the programming people in the computer area had difficulty communicating. We spent a

couple of years trying to get a computer program which would be useful to the toxicologist, without much success. Finally, one programer was able to bring the two groups together because of her ability to understand the toxicologist as well as the programers. When an understanding was reached, we were successful in obtaining a usable computer program which gave the toxicologist what he needed. There is a great need for thorough and informal discussions to arrive at understanding between two groups of scientists trained in biological and non-biological areas. Perhaps a weekly beer party where inhibitions can be diminished is not too bad an idea.

I would also like to point out to Dr. Schneiderman that the problems he would like to have defined for handling by statistics are not always definable in the early stages of biological research. In effect, what we have at the initial stage is a chemical. We may know what its proposed use is, but we may not and probably do not know what it is going to do in terms of adverse effects. Once we have studied the chemical in appropriate tests in a given species of animals, we can define the problem for that species. If we go to another species, the problem may change somewhat, and if we go to man, it may change again. So, you really cannot define the problem except in a general way. We want to know what is going to go wrong when we give this drug to an animal, including man.

Another thing which is a little difficult to define is the "safe dose." Does this mean "no effects," "no adverse effects," "no observable effects," or just what? There is another term that could be used, and that is "risk." Risk is related both to the more or less arbitrary "safe dose" and to the "exposure level." How much of a risk can we take? Another term that is being used is "virtually safe dose" which in essence encompasses both risk and safe dose. These are the things I think we should address ourselves to, but again I would like to expand on the noise question which is our real problem in the area of toxicology. I am sure Dr. Rall recognizes this, and I think Dr. Schneiderman does also. I have two tables which were used last spring at the Academy conference on carcinogenicity testing. Table I lists the variety of tumors that occurred in various organs in control animals. It can be seen that a large variety of tumors can occur spontaneously in a large number of organs. Table II shows the tumors occurring in two concurrent control groups, each consisting of 70 male and 70 female rats. The tumors with the asterisk are tumors which occurred in control animals but not in treated animals. There were a fair number of this type of tumor incidence including a transitional cell carcinoma of the urinary bladder. This finding in control aniamls but not in treated animals would tend to make you wonder about the validity of the cyclamate studies. Also, I would like to point out, under the adrenal gland, the occurrence of pheochromocytomas in both control groups of animals. It is to be noted that a larger number of pheochromocytomas occurred in the male animals of control group I than in the male animals of control group II. Statistically, this is a significant difference. Suppose control group I had been a treated group? How would this be handled? On the surface it would appear that the pheochromocytomas occurring in the one group would have been due to treatment and therefore we had a carcinogenic agent. This is a problem we often meet and this is the reason we are running two control groups in our studies. We recognize this difference, not only in the terms of carcinogenicity, but also in the terms of other toxicologic parameters.

DR. WORCESTER—Well, I'll be very brief, hoping that somebody will have something to say in general discussion. I think that Dr. Rall has dismissed epidemiologic studies perhaps too lightly in certain instances. I'll grant

TABLE I.—Spontaneous Tumors in 1362 Control CRCD Male and Female Rats in 12 Studies (57 to 104 weeks' duration).

Tissue	Tumors
Adrenal	adenoma, adenocarcinoma, pheochromocytoma
Brain	astrocytoma, glioma, neurofibrosarcoma
Cervix	fibroma
Hematopoietic	hemangiosarcoma, leukemia
Liver	adenoma
Lung	lymphosarcoma, non-chromaffin paraganglioma
Kidney	carcinoma, cortical adenoma
Mammary gland	adenoma, fibroadenoma, fibroma, adenocarcinoma, cyst adenoma, papillary cystadenoma, papillary cyst adenocarcinoma
Ovary	granulosa cell tumor, papillary cyst adenocarcinoma
Pancreas	adenoma, islet cell carcinoma, acinar cell adenoma
Parathyroid	adenoma, adenocarcinoma
Pituitary	adenoma, adenocarcinoma
Salivary gland	adenocarcinoma
Sebaceous gland	carcinoma
Skeletal	osteogenic sarcoma
Skin	papilloma, carcinoma, squamous cell carcinoma, trichoepithelioma, basal cell carcinoma, fibroadenoma, keratoacanthoma
Spleen	lymphoma
Striated muscle	fibrosarcoma, fibroma
Stomach	papilloma
Testes	interstitial cell tumor
Thymus	carcinoma, sarcoma, thymoma
Thyroid	adenoma, adenocarcinoma
Urinary bladder	transitional cell carcinoma
Uterus	sarcoma, adenocarcinoma, polyp
Vagina	leiomyosarcoma
Miscellaneous	reticulum cell sarcoma, fibrosarcoma, lipoma, odontoma, liposarcoma, lymphoma, fibroma, sarcoma, lymphosarcoma

TABLE II.—Spontaneous Tumors in Control CRCD Rats in a 96-week Study.

	Control I		Control II	
Sex	F	M	F	M
No. rats examined	70	69	69	69
Organ/Tumor:				
Lung				
non-chromaffin paraganglioma	0	0	1	0
Liver				
hepatocellular adenoma	1	2	1	1
reticulum sarcoma	0	0	1[a]	0
Kidney				
adenoma	0	1	0	0
Urinary Bladder				
transitional cell carcinoma	1[a]	0	0	0
Skin				
adenoma	0	0	1	0
lipoma	0	1	0	0
keratoacanthoma	0	1	0	0
squamous cell carcinoma	0	0	0	1[a]
basal cell carcinoma	0	0	0	1
fibroadenoma	0	0	1[a]	0
Mammary Gland				
adenoma	6	0	6	0
fibroadenoma	11	1	11	0
cyst adenoma	—	—	1	—
papillary cyst adenoma	2	0	2	0
adenocarcinoma	4	0	3	0
papillary cyst adenocarcinoma	5	0	2	0
Uterus				
polyp	2	—	3	—
Ovary				
granulosa cell tumor	1[a]	—	1[a]	—
papillary cyst adenocarcinoma	—	—	1[a]	—
Testes				
interstitial cell tumor	—	1	—	1
Pituitary				
adenoma	23	14	23	11
adenocarcinoma	5	4	5	2
Adrenal				
pheochromocytoma	1	14	5	4
cortical adenoma	1	2	4	1
Thyroid				
adenoma	1	0	1	1
adenocarcinoma	0	1[a]	0	0
Pancreas				
islet cell adenoma	2	4	0	3
acinar cell adenoma	0	0	0	1

(Continued on page 82)

TABLE II.—(*Continued*)

Sex	Control I F	Control I M	Control II F	Control II M
Thymus				
thymoma	0	1	1	1
Spleen				
lymphoma	1	0	0	1
reticulum cell sarcoma	0	0	0	1[a]
Brain				
glioma	1[a]	0	0	0
Skeletal Muscle				
fibrosarcoma	0	0	0	2[a]
Bone				
osteosarcoma	0	1	0	0
myelogenous leukemia	1	0	1	1

[a] Found only in control animals.

there are rare instances they can be carried on successfully, and they don't necessarily take a long time to do. The so-called case controlled studies are ones in which you are able to identify your response in terms of a particular type of tumor. You can get a group of tumors, a group of controls, find out the exposure on the basis of history. These studies of course do get criticized severely in many instances because they are badly done. Occasionally you can do a study that is prospective in the sense that you are able to identify a cohort of individuals to which something happened in the past and follow them up at the present time. Such a study was done on mustard gas where the men were exposed in World War I and the effects were measured in, I think, 1965. So you could get risks in that sort of study. I think also that surveillance might be thought about at least in the sense that now in certain industries union and management are working together and pooling their records in investigations of health of the workers. If it were possible to get decent documentation of industrial processes and their changes, we might get a little information out of that.

Now, I asked a variety of my friends for a definition of threshold, and my toxicological friends all seem to insist that thresholds do exist. Another toxicologist replied that this was a trans-science question with no observable answer; and perhaps after listening to Dr. Schneiderman I think this is not a bad response to this particular question. Regarding numbers of observations in the study, assume one is working in the range where the probability of response, P, is something like one in a thousand. Then q^n is a probability of observing no response in a group of size n, and if we set that equal to .05, we find that n has to be in the order of magnitude of 3000 animals studied at that particular dose in order to be sure of getting some sort of response. This really means that most of our information in the low dose range is going to be obtained by extrapolation from the high dose range. And it is this of course that is getting us into trouble, because if we have only observations of the high dose range, we have no way to predict what the shape of the curve is in the low dose range. It was that sort of extrapolation that got us into trouble in the Cutter incident involving the use of the Salk vaccine. It's dangerous business but it has to be done. I think that anytime that it does have to be done there is some duty to put a confidence interval on the extrapolation so we can at least know where we are.

DR. MANN—During your remarks it occurred to me that giving high doses is analogous to over stress testing or accelerated life testing in the area of reliability, so that some of you pharmacologists or toxicologists might be interested in looking at the literature that's been published in that area. There may be some value in communication between investigators in the two areas.

DR. PECK—I would like to ask one question. In the epidemiological studies you were talking about, I assume you have a definable problem.

DR. WORCESTER—I have a definable response.

DR. PECK—Since you do have a definable response, I think Dr. Rall will agree with me that this makes the epidemiological study much easier.

DR. RALL—Yes.

DR. PECK—There is one problem in epidemiological studies in the industrial situation. Workers may be exposed to a variety of chemicals in different situations because they are so mobile now, and I wonder if they are in one industrial situation long enough for a defined response to be a useful observation.

DR. WORCESTER—There are some kinds of union situations where the employee's record does go along with him.

DR. PECK—There are environmental changes, however.

DR. WORCESTER—Yes, there are environmental changes.

DR. PECK—Then this would be only so useful unless you assume that a chemical produces one type of effect.

DR. WORCESTER—Of course I am not assuming that this is the only kind of study that can be done. I am trying to make the point that epidemiological studies should not be thrown out.

DR. PECK—I agree to that.

DR. RALL—I guess I had better defend myself. I thought after I said that, I made a plea for increased support in epidemiology and if I did want to throw out epidemiological studies I wouldn't suggest we need many more epidemiologists now. The specific point I was trying to address was that in looking for new carcinogens in the environment I think it's much more efficient to study them in laboratory animals first, rather than totally depend upon human epidemiology to identify them. We certainly need the human epidemiological surveillance anyway, because an awful lot of compounds in the environment have never been tested and probably never will. This is one way we can pick them out. But I'll defend epidemiology to my dying day.

DR. PECK—One question I would like to ask you, Dr. Rall. You mention the fallacy of using young healthy animals for the carcinogenic studies. I wonder if this is really wrong. In the human population in industry we are supposedly young and healthy, with exceptions of course, and then as we get older we get a variety of diseases. The same is true of animals. It is true that we don't know all the genetic variations in animals, or at least variations that we can detect. We do run across an occasional animal that has liver disease, diabetes, or some other disease. I am not sure that our procedure is all that wrong.

DR. RALL—No, I don't think it is wrong. I think it is the only thing you can do. But I think when you try to compare groups of animals that have really been picked for prime health at the weanling age or before, they aren't really quite representative of an entire population. Now admittedly these animals will grow, get old, and get sick. You know they're really selected to be in perfect shape. It is all we can do. We simply must, I think, realize this makes it a little harder for simple one-to-one extrapolation.

DR. PECK—We could catch our animals in the wild and use them, but I would not recommend this procedure.

DR. RALL—No, I'm not recommending it. I think you've got to do exactly what you do but realize that it still isn't quite ideal.

DR. SCHNEIDERMAN—I want to comment on the interaction of materials, because almost everybody mentioned this. We've always behaved as if carcinogens operated independently of each other, or at worst were additive. Richard Doll gave a paper at the International Cancer Conference in Houston with a more statistical version published in the Journal of the Royal Statistical Society

in which he reported that in certain industrial exposures, namely the uranium miners and the asbestos workers, the interaction between industrial exposure and cigarette smoking was multiplicative—a very serious kind of interaction which enhanced the hazards. So maybe the James Crowe suggestion that I picked up will not be conservative, if these things interact multiplicatively. We are now doing some work in cooperation with both the statisticians and the biologists, at the Stanford Research Institute in which we are attempting to evaluate interaction. We have a series of materials which we are giving two at a time to an animal, using one of Milton Sobel's group-testing approaches and trying to see whether these materials will give us additive effects, interactions, non-linear additive effects, multiplicative effects, or whether they actually inhibit each other. Some preliminary results are already in. I saw Dr. Newell here from Stanford, who remarked to me that it looks as if we're getting every result you can think of—interactions, straight additivity, and some inhibition. The kind of modeling we are doing derives both from a statistical modeling and from working with the biologist.

Concerning the problem of epidemiology, I'd like to mention a meeting in LaCaravelle, in Guadeloupe, at the Institut de la Vie, which was concerned with formulating a better approach to screening chemical and physical agents that might cause birth defects. This is of consequence to cancer workers because people think there may be some relationship between birth defects and cancer effects. Let me read to you the recommendation from one member of this group. First, do monitoring, using registeries, record linkage etc. to detect sudden increases in time-space occurrence of specific malformations—in other words the epidemiologic approach. Second, assure that there are expert committees who can look at these things. If you don't have a mechanism for people to look at the data, it's not worthwhile getting it.

Third, a very interesting idea and one I would like to hear some of the physicians comment on, when deemed appropriate and when ethical considerations permit—new drugs may be tested by administering them to pregnant women just before an abortion is performed with careful examination of the conceptus for evidence of teratogenicity. Two final laboratory points. Temporize through animal testing; screen chemicals for teratogenic effects. The results may exclude certain useful drugs which are teratogenic in certain species but not in man. These drugs will eventually be cleared for use in man after basic research shows that metabolism in man differs from that which is responsible for the malformation of animals. You will recall that Dr. Rall remarked that this is one of the most important areas in which we don't know much—comparative metabolism. And finally, the last point—encourage research into quick laboratory methods such as tissue culture and the work of DiPaolo and people like him. Notice the order in which these actions came. The conference was concerned with teratogenic effects—things that occur quickly and that you can see quickly. I'm not at all sure that this same order is applicable to problems in cancer. But I thought that these particular ideas were sufficiently important and provocative that I would like to read them to you.

DR. RALL—Relative to metabolism studies, of course, this is being done now. But when it comes to pregnant women with abortions there is a real problem. A number of years ago it was thought we could do this in Sweden when abortions were legal. But apparently there is some political difficulty in Sweden, and certainly we have the political and ethical problem in this country, which is becoming worse, I believe. It would be very useful to be able to do this, but when it will come I don't know. I was at that same meeting at Guadeloupe and I should report to Dr. Schneiderman that the man who

wrote that summary was an epidemiologist and that the overall report of the conference is slightly different. Teratogenic effects really occur very rapidly —suddenly when a compound which was highly teratogenic to man came into the environment there were the good sort of surveillance systems that are being set up now between I guess NCI, CDC and the National Foundation. You would detect it really very quickly with minimal human damage, hopefully. I'm not sure.

DR. SCHNEIDERMAN—Sure you would, Dave. But there is a problem here that the physician faces if he should give a drug to a woman for an illness not covered by the package insert. He must weigh benefit to risk, but if the baby ends up with a malformation and he reports it, he has a legal and a malpractice problem which may make him very reluctant to report the effect. This is true not only in teratogenesis but in all areas of adverse reporting.

UNIDENT.—The system really, I think, can't work. We are getting too specifically into drugs now, which I think is kind of a mistake. The system really can't work the way you describe it with the individual physician reporting. You've got to have surveillance of all babies born in certain hospitals—is the pattern of birth defects remaining constant? And as soon as that pattern changes then you initiate a special effort to find out why.

DR. RALL—If you get into the other types of non-therapeutic chemicals, there is still a problem of ethics whether or not you can really give this to man even as an experimental procedure for metabolic studies. Here you really have to file an IND and have all the control manufacturing and so on data available. I'm not sure how necessary this is, but it's the law. So this creates a real problem in metabolic studies.

MR. WANDS—I would like to ask Dr. Schneiderman about a comment that was included in that report from Guadeloupe. As I remember the phrasing, it was to set up a surveillance system which would indicate trouble as soon as we saw a marked increase in birth defects. Now I maintain that as soon as anyone sees a marked increase, he doesn't need the statistician. It is when we don't see the marked increase and the low-dose-effect-response situation comes into play fully that we need the statistician. This is where we need the kind of guidance you've been giving us already in your professional career, Dr. Schneiderman. I would like to have some detailed comments on what constitutes a marked increase.

DR. SCHNEIDERMAN—We are here among people who are co-sponsors —the quality control society, some of whom I'm sure consider themselves statisticians and whose major business is to indicate to the manufacturer when a sharp increase in defects has occurred —that something has gone wrong. In manufacturing you have a quality control system; you have a time scale, and you can see when you go out of the control limits. I think maybe the quality control people ought to say whether by and large the production people notice those things with or without them. My experience is that the production people usually do not notice those things. In looking at quality control procedures in hospital laboratories in which I have had a little experience, we found very often that the laboratory chief did not know when he had gone out of control. He can have a pretty substantial drift in one of his machines measuring something and he may not notice it for quite some time. Maybe I'm talking about smaller differences that Ralph Wands is talking about. But these are big enough differences so that you might take a different action with the patient. I would see a surveillance system operating like a quality control system. If you don't want a statistician, at least let's have a quality control engineer doing it. "Marked," by the way, is outside the

quality control limits—or the upper limit, at least.

DR. FRED LEONE (ASA)—First of all, I would like to back up Marv Schneiderman's remark, because there are many cases in which, by use of good statistical surveillance, you catch this much before the public or manufacturer would. You would catch drifts before you catch catastrophes, let's say. So, if it is a good system, then it does work. I think you have a lot to gain. I wanted to mention a couple of points and ask for one of the panelist to show a comparison or perhaps tell us what has been done along these areas. This morning Dr. Newill showed us the exposure response matrix itself, saying that, as far as he knows, very little has been done relating or comparing the acute effect, short-term exposure versus the chronic effect, long-term exposure. Another matrix is a high dose for a short term versus a low dose for a very long term. And then finally, as referred to earlier by Dr. Mann, there is an accelerated test in an industrial environment to see what would happen for destruction of a product which might ordinarily take ten years and you want to carry out the accelerated test in a period of one or two weeks. I think these have a lot in common. I would like to have other panelists say what has been done.

DR. SCHNEIDERMAN—In the same set of experiments that I referred to earlier, in which we are testing many materials at the same time, we're also attempting a correlation between early effects, acute effects, medium short-term chronic effects, and long-term chronic effects. Mrs. Maunder is here—she has worked directly on this problem using the Stanford Research Institute data with Bob Ellashoff. With respect to the short burst versus the long-term exposure, a fair number of attempts have been made to do something. It's quite difficult to know what the appropriate dose metameter is in man. Is it the integrated dose that people are exposed to over a long period of time, or is it the peaks of dose that somebody gets at times? Enterline separated his workers into two different groups—production workers who are exposed at a relatively uniform level over time as compared with maintenance and other workers who got bursts of exposure. For the same total integrated dose it looked as if the maintenance workers were at higher risk. That seems to say that bursts are worse, or more damaging.

One last remark relates to something that Dr. Mann had said. There was a seminar this past summer—several weeks at the University of Florida in Tallahassee under Frank Proschan—on reliability and biometry, so that the techniques which were used in both areas might be brought together and discussed. We at the Cancer Institute helped support part of it because we thought that these issues—destructive testing, high level doses, and so on—were important to us. The Proceedings are already available from SIAM (Society for Industrial and Applied Mathematics).

UNIDENT.—You were talking about the type of dosing or type of exposure. Have you looked at the accumulative exposure under these conditions? It has been my impression—I don't know how valid it is—that it is not that you are getting small doses over each day or large doses every other week, but it is the total amount to which you are exposed over a period of time that determines whether you then develop cancer.

DR. RALL—I think there are clearly defined experiments that ought to be done and that haven't been done. These are very real questions. Just to get back to the drug field it poses questions clearly. Suppose you have a drug that is quite effective for myocardial infarction. It is certainly not appropriate to test it for carcinogenicity using weanling or newborn animals because they almost never get myocardial infarctions.

It would be appropriate to test it with a middle-aged animal and so on. But this sort of exploratory examination I think has really not been done or pulled together so that you can see any sort of overall patterns emerging.

UNIDENT.—Well, the oncologists very often use the term "total dosage." Maybe it's just that it develops quicker if you give a few large doses. I just don't know.

DR. MANN—I would like to mention that I too gave a paper at Frank Proshan's Conference on Reliability and Biometry. It wasn't on the subject I very often write about—that is the Weibull distribution. I notice that reprints of many of the papers of mine that concern the Weibull or extreme-value distributions are requested by medical people. It may be that this is a very nice distribution for analysis of pharmacological and toxicological data, particularly because there is a three-parameter Weibull distribution with a threshold parameter. Most of my work is related to the two-parameter form of this distribution. It may be that for some of the things we have talked about today some form of the Weibull distribution is a very good model. One of the things you might do is plot your data on Weibull liable probability paper to see if this may help you in its analysis.

DR. LYLE CALVIN—I'd like to ask Marvin a couple of questions. First of all, he made a statement that if we thought that the statistician was objective we were fooling ourselves, and I think Marv was fooling himself. He fell into the one-sample trap. At least he demonstrated that one statistician may have been fooling himself. There are several references to cost benefit analysis. Marvin, you talked about some of the problems with it. We've been looking at this with respect to a lot of different types of evaluations, and one thing that hasn't been mentioned and bothers us is that cost benefit analysis is basically a one-variant analysis. This is often brought back to a dollar figure by economists. In reality when we look at these costs and benefits, whether we are talking about coal miners or us, the problems are basically multi-dimensional. If you look at it from this aspect, you really have a multi-response surface. It is very difficult with changes of any type to raise the surface at all—to say it is a benefit to society. What we generally do, I believe, is to tip the surface or move it all around, but basically it is going to stay at about the same level. We want to weigh one part of the population. If it tips up this way that's the way we want to make a decision, and then it is a benefit to that particular group of people. But if it tips another way it is a benefit to somebody else. Perhaps this is where the trans-science questions come in deciding how we want to tip that surface. But I would like your reactions to the approach of looking at it as a multi-dimensional problem.

DR. SCHNEIDERMAN—The comment to the first comment is that Lyle is probably correct. Maybe it is just one statistician with one simple who has fooled himself. I wanted to show you how, if I hadn't given you the early data, I might have fooled you too without being corrupt, renal, or nasty. I'm glad Prof. Calvin noted that a cost-benefit analysis based on the single measure of the dollar cost really can get you into great trouble. For example, how should I measure the cost of a life? Should I measure the total lifetime earnings that are not earned? If I do, does the life of a person over the age of 65 have a negative value? Quite obviously, from that kind of cost-benefit analysis, when you reach the age of 65 the most cost-effective thing is to have you executed. Perhaps we ought not to attempt to prevent or cure any diseases, because if we do we'll have lots more people over the age of 65 and we'll have to pay lots more pensions. Quite obviously that simple and simplistic approach is inadequate. I'm very pleased

to hear that Dr. Calvin's group at Oregon are working on this problem because I think some good things will come of it. I'm very much in agreement with him that it has to be considered multidimensional—but different people place different importance on the same data, which then in turn would tip that surface for one person or put bulges or hollows in places for others. If one recognizes that, then I agree one certainly gets into trans-science questions—who is more important? What is more important? How can I evaluate that? Richard Doll once ran away from that one by saying he didn't want to be in a position of comparing the worth of a life of a child with that of a member of the Royal Society. If I were asked to answer that, I would run away too.

DR. PECK—I wonder if there isn't something that you haven't said deliberately—this matter of loss of life isn't as important to me as the suffering that occurs before the individual dies, as well as the suffering and tremendous financial drain on the family and on the community. I think I would rather die suddenly than suffer two or three years.

DR. SCHNEIDERMAN—I hadn't not said it deliberately. I don't know how to say it, or measure it, or evaluate it.

MR. WANDS—I wonder if Dr. Schneiderman would like to comment on the Mantel-Bryan approach that FDA apparently accepted and then went back on. Would you comment specifically on their proposal, and also the general concept if you don't like the number one hundred million or if you don't like a probit of one.

DR. SCHNEIDERMAN—A comment on the general proposal. I think the Mantel-Bryan approach is lovely from the following point of view. It has built-in ways to benefit people who do better research. From that point of view I think it is a marvelous idea. If the manufacturer does more and more animal work at higher and higher doses and his material keeps turning out not to be a carcinogen—not to show any difference from the controls, he then gets to use the material at higher doses. This is clearly to his benefit. I think in that sense this is a very nice approach. Statisticians ought to be working on experimental techniques that have positive payoff to the persons doing the experiment. Most (or all) techniques don't have that kind of thing. I think we have to build in some kind of reward system and the Mantel-Bryan approach does this. I think social and political pressures will help establish a "virtually safe level"—perhaps different from Mantel's 1 in 10^8. Where one uses the probit model or one of the other models should come out of a mixture of biology, rationality and conservatism.

DR. PECK—The Delaney Clause.

DR. SCHNEIDERMAN—I think the FDA proposal was not using the procedure as Mantel had intended. I know Mantel wrote them a letter and objected to their use of it, and that seems to me evidence that he thought they weren't using it his way.

UNIDENT.—A sample size of one, however.

DR. SCHNEIDERMAN—A sample size of one, but I do think it is objective evidence that they weren't using it the way he intended it.

ISRAEL ROCKMAN—I have three comments. First, on the matter of dying rather than suffering. My sister-in-law's mother suffered from diabetes and for the last few years always had a simple reply to her friends when they complained about the pains of old age. She would tell them it was better than the alternative. I also know of at least one lady who, for the past 20–25 years, has been suffering from one ailment or another, starting with some kind of cancer, and who has in the meantime in spite of her

suffering done tremendously useful work both in her professional career and as a humanitarian, so that suffering isn't the only measure of evil. Second, your comment about making comparisons between large doses and small doses using an analogy of testing the destruction. I think the reason for testing for destruction may be more useful in mechanical applications where the mechanism is better understood. So you can more readily make the transfer of information from what happens when you test something severely to the point where it breaks and figure out what might have happened if lesser molds. It is my impression that in biology the mechanisms are usually not that well understood, so the transfer of information would be much more difficult.

DR. MANN—They are not understood in the physical sciences either.

DR. ROCKMAN—I wonder if anybody has every tried to extrapolate from a small animal like a mouse, say, to a dog or to a monkey and stated "This is what I have observed in a mouse—this is what should happen in a dog." Now you can run an experiment with an animal much more readily than you can with people to see whether your method of prediction, your method of transfer, is really correct. Has anybody ever tried this?

DR. PECK—I can give you at least one example. However, regarding your first remark, when I am talking about suffering I really mean suffering. I have diabetes, but I am not suffering and I am still, I hope, productive. There are many people with cancer who are able to tolerate their pain sufficiently that they are still productive, or at least not an undue burden on others.

Now, when you try to extrapolate from mice to monkeys to dogs, I think one remark you made was very important. There is a lot we don't know about biology. We had one compound we are working with for animal science, which in the rat, in the dog, and in the sheep (which was the target animal) produced optic degeneration at high doses. Although much larger doses were given to monkeys, optic degeneration was just not obtained. This was a difference in absorption of the chemical from the gastrointestinal tract with resultant very low blood levels in the monkey. The monkey for some unknown reason just did not absorb the material whereas the other species did. Oddly enough, if the chemical was given in plasma to the monkey, higher blood levels were obtained, but optic degeneration was not obtained probably because sufficiently high doses could not be given. There are species differences which we have to recognize. When we go from animal to man, we have to accept the possibility that man may not be like any of the animals that we have tested. However, as a general rule there is enough similarity so that the extrapolations can be relatively reliable, although not 100% certain. There is probably more of a quantitative difference than a qualitative difference in metabolism. I don't think this has really been examined carefully enough in the past. We are collecting more data now, and eventually we may be able to get a better feel for the reliability of the animal work.

DR. RALL—In the study I referred to very briefly in my talk, we looked at a series of 20 odd anti-cancer drugs tested on mouse, rabbit, hamster, dog and monkey. There were some systematic differences. The rat tended to be rather more susceptible than the other species, and I think this really is because rats weren't very healthy 10 years ago. There is some information on that paper which was published in Cancer Chemotherapy Reports. This was short-term toxicity—not acute but short-term —and this may or may not be relevant to carcinogenicity. Some few years later Phil Schien, who is still at the National Cancer Institute, looked at the qualitative toxicity of a variety of cancer chemo-

therapeutic agents in dogs, monkeys and man. That paper was published in Clinical Pharmacology Therapeutics and is very interesting. I won't attempt to summarize it.

DR. SCHNEIDERMAN—I would like to point out that the authors of that last paper had a real advantage over the usual tester of drugs or chemicals. They worked on anti-cancer agents. They gave toxic doses to man. Thus they were able to get a better correlation than we can in our other types of work.

DR. RALL—Absolutely. You just cannot do it with something like aspirin, or if I may use one of the terms, trivial drugs.

Air Pollutants—Safe Concentrations? Introduction

Henry Lathrop

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Good morning, ladies and gentlemen. Welcome to the Thursday session of Statistics and the Environment. The session this morning is entitled "Air Pollutants —Safe Concentrations?" Your program lists David Solomon as your moderator. Mr. Solomon regrets that he is not able to be with you this morning. He is out of town and I am his deputy in the Environmental Design and Control Division of the Office of Research, Federal Highway Administration. Our work is, of course, related to these subjects and we're very much interested in it.

Before introducing the first speaker, let me introduce two additional panelists. We will hold all questions until after the second speaker has finished. Our first panelist is Dr. William Kirchhoff, who is a physicist and Deputy Manager, Measures for Air Quality, National Bureau of Standards, Gaithersburg. Our second panelist is Dr. Nozer Singpurwalla, Professor of Operations Research, School of Engineering and Science, George Washington University.

Our first speaker this morning is Dr. John Finklea, an MD and a DPH who has had a wide experience throughout the years, having grown up in South Carolina and now the Director of the National Environmental Research Center, EPA at Research Triangle Park, North Carolina. It is a pleasure to welcome you, Dr. Finklea.

Auto Emissions and Public Health: Questions, Statistical Problems, and Case Studies

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Industrial nations now recognize that environmental factors are among the most important determinants of mankind's future physical and economic well-being. The effects of environmental stressors upon human health and the actions necessary to protect public health are areas of public concern and public disagreement. No environmental problem is of greater interest to the average American than the control of emissions from the nation's fleet of more than 100 million vehicles. This report will discuss the problem of determining "safe concentrations" of air pollutants in 3 stages beginning with a general overview

composed of 12 key questions and their answers, progressing to a brief discussion of 6 unresolved statistical problems and ending with a review of short case studies on oxides of nitrogen and on problems engendered by oxidation catalysts. The present communication is not intended to address all of these problems in detail but rather to help the reader better understand the scientific and regulatory challenges faced by his society. The viewpoint expressed is that of a public health physician inextricably entangled in the problems described and the candid observations contained herein do not necessarily represent the official views of any agency.

Twelve Questions

The conceptual and practical scientific and regulatory problems encountered in defining and achieving "safe ambient concentrations" of pollutants emitted from automotive sources can be better understood after we provide the best available answers to the following 12 questions:

- What is a "safe level" of an air pollutant?
- What kinds of information are needed to control an air pollutant?
- How does one compensate for information gaps?
- What are the consequences of unrestrained advocacy?
- How should we assess alternate control strategies?
- What pollutants are emitted from mobile sources?
- What are the major determinants of automotive emissions?
- How well can we measure pollutants in emissions and in ambient air?
- How important are dispersion and transformation?
- How well do emissions controls work?
- How does one link pollutant emissions to human exposures?
- What should a minimally adequate health intelligence base assess?

What is a "safe level" for an air pollutant?

Scholars, scientists, public interest groups, industry and regulatory bodies each approach the definition of a "safe level" with a differing set of biases. As has been pointed out earlier in this symposium, society also lacks consistency in defining safe levels and acceptable risks across the broad range of problems which are regulated to protect the public health and welfare. For ambient air quality our society has established a clear legal directive. The Clean Air Act as amended in 1970 requires that health-related or primary air quality standards be set to protect fully the public health and that these standards contain an adequate margin of safety. This legislation requires that ambient air quality standards protect both specifically susceptible subgroups and healthy members of the population. The Act excludes severely infirmed persons who require an artificial environment. In theory, accelerated mortality of hospitalized or institutionalized patients with severe, pre-existing illnesses might not be an appropriate effect upon which to base a primary air quality standard. In practice, regulatory agencies have duly considered mortality studies. It is well known that the Clean Air Act specified rather demanding time frames for setting and achieving health-related air quality standards and emissions standards for motor vehicles. Many of you may not, however, recall that passage of the 1970 Clean Air Act amendments was preceded by a half century of lightly regarded warnings about health problems attributable to motor vehicle exhausts and 6 years of activity under the more permissive 1965 Amendments. The net result of the latter efforts was a continuing deterioration in urban air quality indices which reflected the impact of automotive emissions. The Congress also provided for careful technical and legislative review of the problems encountered in implementing their legislation. It is my opinion that any review of

the "safe level" problem which isolates the issue from its legislative context is not likely to prove very useful.

At least two other approaches to the "safe level" problem are frequently espoused. They are the cost-benefit approach and a view that argues that any increase over natural background levels of a pollutant is likely to be harmful and therefore either total prohibition of pollutant emissions or maximum achievable controls should be instituted. The cost-benefit approach attempts to balance control costs against the health benefits. Such an approach is superficially attractive but it is difficult to apply. Basically it is much easier to calculate control costs than to develop the needed health damage functions. With our present limited health intelligence base and with the present methodologic difficulties in assigning and apportioning health costs, there would be a tendency to underestimate true health costs. A cost-benefit approach will require rather precise dose-response functions for each adverse effect. Such functions cannot be constructed in the next few years without a greatly increased research effort. In my opinion precipitous movement to a cost-benefit philosophy would tend to slow drastically the air pollution control effort and probably ignore a large but as yet poorly defined residual of continuing ill health. It has always seemed to me paradoxical that my colleagues who argue most forcefully for applying the cost-benefit approach in the immediate future are most often not the same individuals who support efforts to conduct the health research necessary to generate reliable health damage functions. Indeed, it often seems that advocates of the more stringent "threshold" approach are more likely to understand the need for a better health information base.

The third approach is to require total prohibition of pollutant emissions or maximum feasible controls. Among the advocates of this course there is great disagreement on how to define "feasible." Those pursuing the maximum control approach often argue that any increase in pollutant levels over natural background concentrations is harmful to the public health and welfare. The probable harmful effect of low background levels or seasonal swings in familiar environmental stressors upon especially susceptible subgroups is often cited as justification for the total prohibition or maximum feasible control approach. While it may be theoretically attractive to agree that any environmental stressor can adversely affect susceptible subgroups, there is little practical information to support this position. In fact, it is increasingly difficult to measure adverse effects upon susceptible subgroups as one approaches the primary ambient air quality standards for motor vehicle related pollutants. One also encounters tactical advocacy of the maximum feasible control philosophy among a few who really do not intend to support either the necessary efforts to develop the control technology leading to stringent emissions reduction or the necessary health research to estimate which adverse effects are really "no-threshold" problems. In fact the latter individuals are sometimes alleged to be interested in "cosmetic controls" which take advantage of a limited, technical information base to advocate measures which do little more than minimize "first costs" to special interest groups.

There may also be substantial room for honest disagreement on what constitutes an adverse health effect. In this case the problem usually involves deciding which changes in bodily function represent an increased risk for future disease or for aggravation of existing disorders and which changes are simple adaptive functions. Points of dispute are not easily resolved because pollutant exposures are not usually the sole cause of death or the sole cause of any single disease or group of disorders.

What kinds of information are needed?

Ambient air quality standards rest upon a broad interlocking scientific information base. Weaknesses in one or more of these knowledge areas may severely constrain efforts to establish a health-related air quality standard or to reduce the levels of ambient air pollution. Realistic assessment of our current information base shows that major gaps exist for each of the pollutants covered by the primary ambient air quality standards.

Scientifically defensible air pollution controls require adequate measurement methods for sources and for ambient air, emissions profiles with sufficient temprospatial detail to accommodate implementation planning, a reasonable understanding of pollutant transport and transformation in the atmosphere so that one can quantify the determinants of secondary pollutants generated in the atmosphere, a good air monitoring data base, dose response information linking pollutant exposures to adverse effects on health and welfare, predictive models linking emissions to air quality and to adverse effects, and viable control technologies. Without this information we must deal with major uncertainties and run substantial risks of instituting less than optimal control strategies.

How does one compensate for information gaps?

How does one compensate for information gaps when faced with legislative mandates that include demanding schedules for the promulgation of standards and institution of control measures? At least 3 options are available. In my opinion the first and most reasonable option is to initiate the required regulatory actions and to define the range of uncertainty, assess its importance and initiate the necessary research program to reduce uncertainty to tolerable levels. A second option which may be used along with the first is to include margins of safety in health-related standards to compensate for uncertainties. When one is dealing with stringent controls of emissions, safety margins are an expensive way to compensate for uncertainty. A third option is to make decisions that either ignore uncertainty or employ uncertainty in an asymmetrical fashion to support particular decisions while making serious efforts to do the research necessary to gain a more complete understanding of the problem. This brings us face to face with another question: how much information is enough? Frankly, it has been my experience that the answer is more economic than health-related. Controls that affect major, tightly organized industrial enterprises are likely to require the greatest technical justification over and above that required by any postulated adverse health effects attributable to the problem. From a public health point of view the amount of information required for a control action would depend upon the ubiquity and intensity of exposure, the severity and frequency of adverse health effects, the likelihood of interactions intensifying the effect of other major determinants of ill health and the ability of existing technology to assess the problem in question. There may be occasions when health priorities and economic priorities do not coincide.

What are the consequences of unrestrained advocacy?

Scientists and the legal profession must learn to understand each other and to work together towards solving environmental problems. Working together does not mean assuming a posture of unrestrained advocacy. In fact, unrestrained advocacy complicates and can impede efforts towards rational solutions. Scientists and lawyers should be seeking ways to make available all relevant information developed by government, industry or other groups rather than pursuing unrestrained advocacy. How much could information hidden in the files of industry and government assist our national effort to achieve rational environmental controls? I do not know the answer to this question but it is apparent that the information needs are

very large and that our nation should strive to optimize its use of limited research data resources. There is also substantial danger that unrestrained advocacy might channel limited research resources into supporting an advocacy position and not towards reducing the most important areas of uncertainty. Finally, a long series of adversary actions focused on existing problems may cause us to overlook emerging problems that can be ameliorated or avoided if our efforts are more properly focused. On the other hand, there can be no doubt that poorly targeted, meandering scientific efforts not focused on adequate legal mandates will also fail to serve the best interests of our nation. We need to develop mutual understanding between scientists and lawyers to ascertain how we can attain clearly defined goals subscribed to by a consensus of our society.

How should we assess alternate control strategies?

The Clean Air Act established precise goals for the reduction of automotive emissions but allowed somewhat more flexibility in the time allowed to attain these emission reductions. Several alternate control strategies have been proposed or informally discussed by scientific panels, industrial concerns, other governmental units and by public interest groups. Usually one does not have the minimal information base necessary to evaluate alternate proposals. For each alternate control strategy one should ask and answer the following questions. What time frame is envisioned? What is the impact on fuel economy? What capital investments and consumer costs are involved? How effective is the proposed strategy when compared to other strategies? How do controls influence emissions not currently regulated? And finally, what hazards may be produced by the control strategy itself? In other words, one must understand the impact of controls not only on air quality and the economy but on overall environmental quality and public health as well. Neither the Federal government nor American industry has done a good job with these problems. Serious questions are often raised about proposed controls early in their development but these problems are too often inadequately addressed until large investments have been made. Serious current problems in both stationary and mobile source control programs could have been avoided if the environmental and public health impacts of proposed control strategies had been carefully considered. Controls based upon a fragmentary understanding of the problems one wants to correct seem the most likely to themselves produce large problems.

What pollutants are emitted from mobile sources?

The Clean Air Act specifically mandated reductions in emissions of gaseous hydrocarbons, oxides of nitrogen and carbon monoxide from light duty motor vehicles. The Act also contained specific provision for later regulation of exhaust particulate levels if this was deemed advisable and for the regulation of fuel additives and fuel composition. The Environmental Protection Agency has proposed regulations which will reduce the amount of lead in gasoline and require registration of both fuels and additives to fuels and lubricants. Small amounts of sulfur oxides and certain metals are also emitted from current vehicles. Complex exhaust particulates are usually composed of a core of lead or other metals and a shell of complex hydrocarbons. These particles are small enough to penetrate deeply into the lung. The hydrocarbons emitted and the resulting hydrocarbon shell which is formed around particulate nuclei contain a number of carcinogens and co-carcinogens. Use of fuel additives or lubricants containing other metals, for example manganese, would undoubtedly alter the structure of exhaust particulates. Emissions control systems used with current automotive power plants and the introduction of alternate power systems can further alter emissions profiles. For

example, gas turbines might emit worrisome quantities of certain nickel compounds, and stratified charge and diesel engines might emit greater quantities of poorly characterized exhaust particulates. Emissions control systems also can change complex gaseous hydrocarbon profiles.

What are the major determinants of auto emissions?

Major determinants of emissions include certain characteristics of the vehicle population (age, size, power plant and growth rate), driving habits and patterns, vehicle maintenance, performance and deterioration of emission control devices, and certain characteristics of the fuels, fuel additives and lubricants utilized.

Vehicle populations, like human populations, have characteristics which vary from place to place and over time. There is currently a shift towards smaller vehicles and one may see a trend towards a longer survival of older, uncontrolled vehicles because of their somewhat greater fuel economy and possibly as a result of economic dislocations. The introduction of alternate power plants would also change emissions patterns. An even more important influence is the growth rate. Different assumptions for the growth of the vehicle population will lead to markedly different projections at the end of a decade but will not greatly influence projections for 1, 2 or 3 years. Energy constraints might be expected to slow the rate of growth for the vehicle population. Since stationary and area sources contribute significantly to hydrocarbon and nitrogen oxide emissions, the relative importance of these sources to mobile sources should be kept in mind and the expected variations of each over time considered.

Energy shortages may well change driving habits sufficiently to alter emissions projections in that the number of cold starts and the number of vehicle miles driven will probably be reduced. However, if there is a larger reduction in vehicle miles than number of trips, one may well find that the number of cold starts has been only minimally reduced. Since cold starts account for a disproportionately large fraction of the emissions measured during the currently used test cycle, adjusting emissions estimates on the basis of reductions in vehicle miles traveled overstates the effectiveness in reducing emissions. Reducing top cruising speeds on arterial thoroughfares from 60 or 70 to 50 mph will increase emissions of carbon monoxide and hydrocarbons but decrease emissions of nitrogen oxides. Overall it is difficult to predict the exact effect of likely changes in driving habits engendered by our energy shortages. In general, the direction would be to reduce emissions, especially in non-urban areas, but the reduction would not be proportionate to the reduction of vehicle miles traveled.

In the hands of consumers, control devices may be intentionally circumvented or simply deteriorate because of improper maintenance. A great deal of uncertainty surrounds the overall impact of control device deterioration. Obviously, this factor will greatly influence emissions projections.

Energy or regulatory constraints may also lead to alterations in fuel composition that could influence the reactivity of hydrocarbon emissions and the emissions of presently unregulated complex organic compounds. Similarly, fuel composition and fuel additives can affect the performance of emission control devices. The impact of such changes on each emissions category is not easy to predict at the present time.

How well can we measure pollutants in emissions and in ambient air?

In general methods for exhaust emission measurements constitute less of a problem than measuring pollutants at the lower concentrations which are found in ambient air. It is also less difficult to characterize and measure accurately a primary pollutant like carbon monoxide than transformation pollutants like the

nitrogen dioxide, ozone, other oxidants and the aerosols derived from nitric oxide and sulfur oxides emissions. A final general observation is that measurement methods can always be improved. It is our opinion that measurement methods for exhaust emissions of carbon monoxide, gaseous hydrocarbons and nitrogen oxides are reasonably adequate for current vehicles but more sensitive methods may be required to monitor vehicles that meet statutory standards. Usable methods also exist for measuring emissions of exhaust particulates and metals. On the other hand, better methods are needed for exhaust aerosols and more complete characterization of exhaust particulates, aerosols and hydrocarbons is required for both exhaust streams and ambient air. Adequate methods also exist for measuring carbon monoxide and ozone in ambient air. Our present Federal Reference Method for measuring non-methane hydrocarbons in ambient air is not sufficiently sensitive as the lower detectable limit of the level approximates the ambient standard instead of being sensitive enough to measure levels only one-tenth the standard, as would be preferable. This difficulty is not of monumental importance as the ambient hydrocarbon standard is at present used only for planning purposes in the control of oxidants and not as a standard enforced because of adverse health effects attributed to hydrocarbons per se.

How important are dispersion and transformation?

Atmospheric processes profoundly affect ambient air quality. Certain pollutants like photochemical oxidants, nitrogen dioxide, acid aerosols and fine particulate sulfates and nitrates arise principally through atmospheric transformations. The relationship between oxidant precursors and oxidants is a crucial area of uncertainty. This complex relationship can lead to situations that alter reactivity in such a way that oxidant levels at a central city monitoring station could be reduced while oxidant levels at downwind sites on the urban fringe might remain elevated or even increase. There is also substantial disagreement among reputable scientists on the importance and extent of regional variations in the formation of secondary pollutants. Present control plans recognize that background levels of pollutants exist but temprospatial differences in background levels are not considered and no provision has been made to consider the transport of pollutants from one air quality control region to another. Failure to address these emerging problems greatly increases the uncertainty of the efficacy of control strategies.

Other exposures like carbon monoxide and lead derived from fuel additive combustion are maximal where vehicles are concentrated. Use of vehicles equipped with catalytic converters will shift acid aerosol exposures into this category. Likewise, widespread use of manganese additives or turbine seals sloughing nickel could create this type of exposure problem. Such pollutants will reach their highest levels in urban street canyons, along arterial thoroughfares and around complex sources like shopping centers and sports complexes. Meteorologic factors are responsible for dispersing and diluting pollutants which are emitted directly from vehicles or formed in the atmosphere from precursor pollutants emitted from vehicles. Meteorological factors are especially important when considering the frequency and magnitude of short term peak exposures. Most existing analyses have assumed that meteorological factors do not vary appreciably from year to year and have not considered the influence of regional and seasonal differences in altitude, sunlight, temperature, humidity and other parameters.

How well do emissions controls work?

Beginning with the 1968 model year, each new cohort of motor vehicles was equipped with some sort of emissions control system as required by Federal

regulations. Thus far emissions controls might be expected to have their greatest impact on carbon monoxide and hydrocarbons with only a modest reduction in nitrogen oxides. Has ambient air quality improved? Our monitoring data are not adequate to answer the question definitively but several hopeful trends are evident. California monitoring data show a reversal of previously upward trends and a substantial reduction in daily maximum carbon monoxide levels. Similarly, peak hourly oxidant levels in cities participating in the EPA continuous air monitoring program have been reduced by roughly 25%. Thus far there are inadequate data to evaluate the effects of recent fuel restrictions on urban air quality. Sufficient information does exist to lead us to conclude that performance of emissions control systems on consumer operated vehicles does deteriorate significantly.

How does one link pollutant emissions to human exposures?

Models linking emissions to human exposure are helpful but crude and often unvalidated. Major problem areas include the influence of human activity patterns, indoor air pollutant levels at home and at work, and our limited understanding of the processes that transform primary pollutants into secondary pollutants. When pollutant exposures involve a large area as in the case with oxidants the problem of constructing an appropriate exposure model will probably prove manageable. Progress is also possible when exposures are largely determined by the proximity of substantial numbers of vehicles, as is the case with carbon monoxide. Then human activity patterns become most important. Control strategies have not adequately considered how human activity influences exposure to carbon monoxide. This failure is especially important because urban exposures manifested by carboxyhemoglobin levels in nonsmokers who donate blood reflect higher carbon monoxide exposure than one would expect from most existing air monitoring data. The most complex situation occurs when emissions from different types of sources and exposures via several environmental media are involved, as is the case with lead. To date only a limited number of investigators have reported studies in which they attempted to establish and utilize human exposure models. Much more work remains to be done if the more obvious major uncertainties are to be reduced.

What should a minimally adequate health intelligence base assess?

A minimally adequate health intelligence base should ascertain the effects of long-term low level exposures and the effects of single or repeated short-term exposures. In general it is easiest to ascertain what acute effects follow short-term fluctuations in air quality. Less complete information is available on the acute and chronic effects which follow long-term low level exposures and very little is known about the chronic effects of peak exposures. The present primary air quality standards usually consider only an annual average or a single short-term averaging time. It is assumed that the necessary air quality controls will also protect against repeated short-term exposures that are less than the standards. This is an untested assumption and further refinement of the standards may prove necessary.

All reasonably expected adverse health effects should be considered when setting a standard. In fact, adverse effects which are postulated but not proven have not always been carefully considered. Failure to consider what is reasonably expected but not yet elucidated ignores a large important area of uncertainty. The effects of air pollutants on respiratory cancers, on the unborn infant and on aging represent three areas of great uncertainty.

The most important expected interactions with other pollutants and with other major determinants of each adverse effect should be determined. In practice,

standards based upon community studies do consider pollutant interactions and from a combination of research approaches one may at times assess the relative importance of other determinants of disease. In the case of the automotive pollutants such assessments have not been completed.

Most adverse health effects are best evaluated by blending complementary research approaches. Epidemiology, clinical research and animal toxicology each have their advantages and limitations. Epidemiologic studies are set in the real world and thus allow consideration of the effect of complex long- and short-term pollutant exposures on susceptible segments of the population. However, community studies utilize rather crude health measurements. They must cope with a host of strong covariates and are restricted to a limited range of exposures. Clinical studies utilize more sophisticated health measurements and carefully controlled exposures of human volunteers. Susceptible segments of the population may be studied and many of the bothersome covariates found in community studies may be avoided. However, long-term exposures cannot be easily evaluated. Toxicology studies provide the opportunity to control strong covariates carefully, to utilize a wide range of pollutant exposures and to examine body tissues. Unfortunately, differences between species and lack of appropriate laboratory models for all susceptible segments of the population limit the usefulness of animal studies. Thus, it is apparent that all 3 research approaches may be necessary and that the design of these studies should provide biological bridges between them in terms of exposure levels considered and health indicators utilized. It is rare that this blend of information can be found.

The present information base does not allow construction of good exposure-response functions for each adverse effect. In fact, we must candidly admit significant uncertainties in our estimates of the effects thresholds for each adverse effect associated with each currently regulated ambient air pollutant. Realistically, the best we can do at present is to define "lower boundary," "upper boundary," and "best judgment" estimates for each "no effect" threshold estimate. Hopefully, these 2 boundary assumptions would provide limits for the arena in which reasonable men might disagree. That is, there should be general agreement that pollution levels higher than the upper boundary assumption result in a particular adverse health effect.

Susceptible population segments subject to greater risk include persons with pre-existing diseases which may be aggravated by exposures to elevated levels of pollutants in the ambient air. Some quantitative information is available on the aggravating effects of air pollutants on asthma, chronic obstructive lung disease and chronic heart disease. One could be legitimately concerned about the aggravating effect of air pollutants on a number of other susceptible population segments: persons with hemolytic anemias, patients with cerebrovascular disease, persons with malignant neoplasms, premature infants and patients with multiple handicaps. Little quantitative information exists about the aggravating effect of pollutants on these disorders.

Air pollutants may also increase the risk in the general population for the development of certain disorders. Many if not all of the general population may experience irritation symptoms involving the eyes or respiratory tract during episodic air pollution exposures. Similarly, even healthy members of the general population may experience impaired mental activity or decreased physical performance after sufficiently high pollution exposures. The general population, especially families with young children, is almost universally susceptible to common acute respiratory illnesses including colds, sore throats, bronchitis and pneumonia. Air pollutants can increase either the frequency or severity of these disorders. Personal air pollution with cigarette smoke, occupational exposures to irritating dusts and

fumes and possibly familial factors increase the risk of developing chronic obstructive lung disease and respiratory cancers in large segments of our population. Air pollutants can also contribute to the development of these disorders. A few animal studies indicate that air pollutants may also accelerate atherosclerosis.

Examples of Major Unresolved Demographic and Statistical Problems

At least 6 major unresolved demographic and statistical problems hamper efforts to control air pollutants. First, the population at risk should be characterized more precisely. Gross estimates are available for that portion of the general population exposed to elevated levels of one or more air pollutants but much better estimates are needed. A key missing parameter is more accurate assessment of temprospatial variation in air quality for automotive pollutants. Susceptible subgroups within the general population must be identified and better characterized by rational groupings of clinical diagnoses which are located, quantified and described by age, sex, ethnic group and socio-economic status. Defensible health damage functions will require much better information about populations at risk. A second problem involves improving our vital statistics and air monitoring data base so that one can assess the effect of short-term fluctuations in air pollutant levels or fluctuations in daily mortality. At present there is a hiatus of several years in the national vital statistics base needed for this effort. A third problem involves improving the classification of outpatient illnesses so that selective morbidity indices based upon outpatient records can become an integral part of environmental monitoring systems. With few exceptions, the usual types of available morbidity data are difficult to utilize because of nomenclature problems, physician variability, difficulty in specifying denominators for rates and problems in assessing pollutant exposures.

A fourth problem already briefly mentioned is that of developing improved models for estimating past, current and future exposures. Recapitulating prior exposures in a mobile society is especially troublesome as is following cohorts of mobile individuals through rapidly changing exposures. Another vexing facet of the exposure problem is developing techniques to overcome problems posed by a single environmental station and multiple respondents or health sensors. One question of dispute in such cases is whether the respondents should be considered as individual or as a grouped observation. A fifth challenge is to develop improved statistical techniques to deal with repeated measurements on the same subjects, that is the problem of inter-correlated multivariate time series. A final closely related need is to improve statistical techniques to deal with intercorrelated independent variables in health studies.

The Case of Nitrogen Oxides

Nitric oxide emissions from both stationary and mobile sources have increased during the last seven years as a result of growth and as a result of early emissions controls on light duty motor vehicles which reduced carbon monoxide and hydrocarbon emissions but allowed nitrogen oxide emissions to increase. Atmospheric processes transform nitric oxides into two pollutants, nitrogen dioxide and suspended particulate nitrates, that are considered public health problems. Nitrogen oxides also enter into photochemical reactions that produce and scavenge ozone and other photochemical oxidants. Major residual uncertainties which hamper control efforts involve health effects, measurements methods and air monitoring. The atmospheric chemistry of nitrogen oxides, modeling problems and control technology are technical areas that also require more work but they will not be discussed.

Health Intelligence Problem

The limited health intelligence base for nitrogen dioxide leaves little doubt that

long-term exposures and repeated short-term exposures to elevated levels of nitrogen dioxide can increase susceptibility to acute respiratory illness and increase the risk of chronic lung disease. There is also ample reason to suspect that other oxides of nitrogen including nitrous acid, nitric acid and suspended particulate nitrates will adversely affect health. Acid aerosols and finely divided particulate nitrates would be expected to aggravate asthma and exacerbate the symptoms of chronic heart and lung diseases. Of equal concern is the possibility that acid aerosols, nitrites and nitrates might increase the risk of respiratory and perhaps gastrointestinal cancers. Actually, there are only a few relevant studies of these problems (see Table 1). There are so many missing pieces in the health data puzzle that one cannot be assured that the present ambient standard protects against the most severe adverse effects. Furthermore, most of the studies upon which the standard is based are community studies. Without accompanying clinical and toxicological studies, community studies usually remain suspect. The other types of studies are required to give a biologically coherent picture and more adequate dose-response relationships.

There is no short-term Federal air quality standard for nitrogen dioxide. Empirical distribution models for cities with continuous air monitoring stations show that the present annual average standard for nitrogen dioxide is roughly equivalent to a 1-hour level of 1400 $\mu g/m^3$. Even this extremely high value is substantially below the best judgment estimates for adverse effects (excluding odor) following short-term exposures (Tables 2 and 3).

Best judgment and boundary estimates

Table 1.—Adverse effects which might be attributed to nitrogen dioxide exposures.

EXPECTED EFFECT	RESEARCH APPROACH: EPIDEMIOLOGY	CLINICAL	TOXICOLOGY AT LOW EXPOSURE LEVELS (<9000 $\mu g/m^3$)
INCREASED SUSCEPTIBILITY TO ACUTE RESPIRATORY DISEASE	THREE REPLICATED STUDIES	NO DATA	REPLICATED RODENT STUDIES
INCREASED SEVERITY OF ACUTE RESPIRATORY DISEASE	TWO REPLICATED STUDIES	NO DATA	TWO STUDIES WITH RODENTS
INCREASED RISK OF CHRONIC RESPIRATORY DISEASE	TWO STUDIES SHOW A WORRISOME FINDING OF REDUCED VENTILATORY FUNCTION IN CHILDREN	ANECDOTAL CASE REPORTS	FOUR STUDIES WITH RODENTS
AGGRAVATION OF ASTHMA	ONE STUDY SUGGESTS PARTICULATE NITRATES AGGRAVATE ASTHMA	NO DATA	NO DATA
AGGRAVATION OF HEART AND LUNG DISORDERS	NO DATA	NO DATA	NO DATA
CARCINOGENESIS*	NO DATA	NO DATA	NO DATA
FETOTOXICITY OR MUTAGENESIS	NO DATA	NO DATA	NO DATA

*THROUGH NITRATES OR NITRITES.

Table 2.—Best-judgment exposure thresholds for adverse effects due to nitrogen dioxide (short term).

EFFECT	THRESHOLD, $\mu g/m^3$
DIMINISHED EXERCISE TOLERANCE	9400 FOR 15 MINUTES
SUSCEPTIBILITY TO ACUTE RESPIRATORY INFECTION	2800 FOR 2 HOURS*
DIMINISHED LUNG FUNCTION	3800 FOR ONE HOUR
PRESENT STANDARD	EQUIVALENT TO 1400 $\mu g/m^3$ FOR ONE HOUR

*BASED ON ANIMAL STUDIES ONLY.

for long-term nitrogen dioxide exposures (Tables 4 and 5) are complicated by the need to consider a variety of averaging times. The situation is further clouded by the pivotal nature of community studies conducted in Chattanooga in neighborhoods near the Volunteer Army Arsenal Plant which emitted acid aerosols as well as nitrogen dioxide. Within the uncertainties posed by the available health studies, the existing standard seems adequate with a margin of safety greater than the margin for sulfur oxides and suspended particulates.

Fortunately, other laboratory, clinical and epidemiology studies on the effects of nitrogen dioxide are becoming available. Each of these is needed to improve our scientific information base and all of these studies have thus far indicated that there is a real need to control ambient levels of nitrogen dioxide. If our strong suspicions about the adverse effects attributable to acid aerosols and suspended nitrates are confirmed, more stringent control of nitrogen oxides may be required to protect public health.

Measurement Method Problem

When the Air Quality Criteria Document for Nitrogen Oxides was issued there was no acceptable method for demonstrating the equivalency of two or more measurement methods. The 2 most frequently utilized measurement methods for ambient air were the continuous Griess-Saltzman method and the Jacobs-Hochheiser method which utilized a 24-hour bubbler system. Both methods were internally consistent but they did not agree well with each other. Because the National Air Sampling Network and a series of key health studies utilized the cheaper Jacobs-Hochheiser 24-hour bubbler method, this method was designated as the Federal Reference Method for nitrogen dioxide measurement. Unfortunately, when adequate permeation tubes became available, our laboratories found that the Federal Reference Method was not acceptable because of a variable collection efficiency. This finding required that the Agency designate acceptable alternate monitoring methods and reassess the primary ambient air quality standard for nitrogen dioxide.

When the original Federal Reference Method was retracted, 3 tentative candidate methods were proposed to serve during an interim period while all candidate methods were being thoroughly evaluated. These candidate methods are the continuous chemiluminescent method, the continuous Griess-Saltzman method and the 24-hour arsenite bubbler method. The latter 2 methods depend upon the same diazotization reaction but differ in the pH of the collection media, the elapsed time prior to analysis and the use of a stabilizing agent. The present Air Quality Standard for nitrogen dioxide is based upon an annual average pollutant concentration and both continuous and short-term integrated methods (e.g., 24-hour bubbler methods) can be used to demonstrate achievement of the annual standard. However, if an air quality standard based on a shorter term of exposure is adopted, then a continuous monitoring method will be needed to measure compliance. In that case the 24-hour bubbler methods, which are cheaper and easier to operate, can be used to identify problem areas requiring continuous monitors and to satisfy some implementation plan needs.

Let me briefly summarize our current information about the measurement of nitrogen dioxide. First, the recently retracted Federal Reference Method which assumed a constant collection efficiency of 35% is not tenable because the true collection efficiency is very high at low concentrations of nitrogen dioxide and

Table 3.—Threshold estimates for adverse health effects attributable to nitrogen dioxide (short term).

Adverse Effect	Research Approach	Type of Estimate	Exposure		Safety Margin ( )* Contained in Primary Standard (1400 ug/m^3 equivalent)
			Level ug/m^3	Duration	
Odor Perception	Clinical	Worst Case	225	5 minutes	None
		Least Case	835	3 minutes	None
		Best Judgment	225	5 minutes	None
Diminished exercise tolerance	Clinical	Worst Case	1900	15 minutes	36**
		Least Case	14500	6 hours	936**
		Best Judgment	9400	15 minutes	571**
Susceptibility to acute respiratory infection	Toxicology	Worst Case	2800	2 hours	100
		Least Case	28200	3 hours	1900
		Best Judgment	2800	2 hours	100
Diminished Lung function	Clinical	Worst Case	3000	1 hour	114
		Least Case	9400	15 minutes	571**
		Best Judgment	3800	1 hour	171
Fatality	Toxicology	Worst Case	165,000	4 hours	11,685
		Least Case	940,000	1-2 hours	67,043
		Best Judgment	188,000	1 hour	13,329

*Safety Margin = Effects threshold minus standard divided by standard x 100.
**Assumes hourly exposure at same level

Table 4.—Best-judgment exposure thresholds for adverse effects due to nitrogen dioxide (long term).

EFFECT	THRESHOLD, μg/m³ *
INCREASED SUSCEPTIBILITY TO ACUTE RESPIRATORY INFECTION	188
INCREASED SEVERITY OF ACUTE RESPIRATORY DISEASE	141
INCREASED RISK OF CHRONIC RESPIRATORY DISEASE	470**
DECREASED LUNG FUNCTION	188
PRESENT STANDARD	100 μg/m³ ANNUAL AVERAGE

*ANNUAL AVERAGE EQUIVALENT.

**BASED SOLELY ON ANIMAL STUDIES.

quite low at high concentrations (Fig. 1). Since nitrogen dioxide concentrations may vary a great deal during the 24-hour sampling period there is no easy way to adjust for a variable collection efficiency over a 24-hour sampling period. Ignoring the latter problem, the usual result of the variable collection efficiency error would be to underestimate the true exposures at concentrations greater than 120 $\mu g/m^3$ and overestimate exposures at lower levels. In general the shape of the collection efficiency curve suggests that the overestimation problem would be more severe. Another problem is that nitric oxide has proved to cause a significant positive interference with the retracted method.

Our laboratories are evaluating 5 other measurement methods including the 3 tentative candidate methods previously mentioned. This evaluation should allow our Agency to designate a scientifically defensible measurement method and to relate that method to the continuous Saltzman method and to the arsenite bubbler method. The latter task is necessary because the Saltzman method was employed in many of the health studies upon which the primary standard was based and because the major portion of our meager national air monitoring data base depends upon the arsenite method. In brief, it seems that the continuous Saltzman method may have problems in that measurements at low ambient concentrations are unreliable and ozone exerts a worrisome negative interference. A number of investigators outside of government disagree with us and feel that the Saltzman method is quite reliable. The arsenite bubbler method has a stable 85% collection efficiency over a wide range of nitrogen dioxide concentrations. However, interferences caused by gases commonly present in urban air handicap this method: carbon dioxide causes a positive interference and nitric oxide a negative interference. These worrisome interferences vary depending on the absolute concentrations of the interfering gases and the ratio of their concentration to that of nitrogen dioxide. The triethanol amine guaicol sulfite (TGS) method appears quite promising even though it is not one of the 3 proposed candidate methods. The TGS method has a stable 93% collection efficiency. No interferences caused by ambient pollutants have been identified and the collection media has good stability after sampling. The continuous chemiluminescent method avoids many of the problems inherent in wet chemical procedures but most instruments thus far evaluated either suffer from early production problems or require highly qualified field operators. However, the chemiluminescent approach retains a great deal of promise. To establish a new reliable reference method which is properly standardized and field tested will require another year with collaborative field testing occupying at least 6–9 months.

Air Monitoring Data

When nitrogen dioxide levels in ambient air were measured at 196 sites using two 24-hour bubbler methods, the former Federal Reference Method and the arsenite method, it was apparent that the Federal Reference Method, which assumed a constant 35% collection efficiency, resulted in readings that were more than twice as high as those obtained by the arsenite method with an assumed constant 85% collection efficiency. This relationship was observed in several sites that had annual average arsenite readings which were just below or just above the primary ambient air quality

Table 5.—Threshold estimates for adverse health effects attributable to nitrogen dioxide (long term).

Adverse Effect on Human Health	Research Approach	Type of Estimate	Exposure: Level ug/m^3	Exposure: Duration	Safety Margin ( )* Contained in Primary Standard: Annual Average Equivalent	Safety Margin ( )* Contained in Primary Standard: (100 ug/m^3)
Increased suscep-tibility to acute respiratory infection	Epidemiology	Worst Case	188 564 376	For ten percent** of hrs. or days for 3 yrs. or less	(94) (282) (188)	None 182 83
	Toxicology	Worst Case Least Case Best Judgment	940 9400 940	3 months 3 months 3 months		20 to 400 9300 50
Increased severity of acute respira-tory disease	Epidemiology	Worst Case Least Case Best Judgment	188 470 282	For ten percent** of hrs. for at least 1 year.	(94) (235) (141)	None 135 41
	Toxicology	Worst Case Least Case Best Judgment	940 3760 940	For 6 or more hrs. each day for 3 or more months		840 3660 840
Increased fre-quency of chronic respiratory disease symptoms	Epidemiology	Point Estimate	No increase after 3 years exposure to levels between 188 ug/m^3 and 564 ug/m^3 on ten percent of hours or days		Not Applicable	
	Toxicology	Worst Case	940 3800 940	For 6 hrs. or more each day for 3 months or more	(470) (1900) (470)	370 1800 370
Decrease Lung Function	Epidemiology	Worst Case Least Case Best Judgment	188 564 376	For ten percent of hrs. for 3 years or less	(94) (282) (376)	None 182 276
Aggravation** of Chronic heart and lung diseases			NO DATA			
Carcinogenesis**			NO DATA			
Fetotoxicity and** Mutagenesis			NO DATA			

*Safety Margin = Effects threshold minus standard divided by standard x 100.
**Through either nitrogen dioxide or nitrous acid - nitric acid - nitrate route.

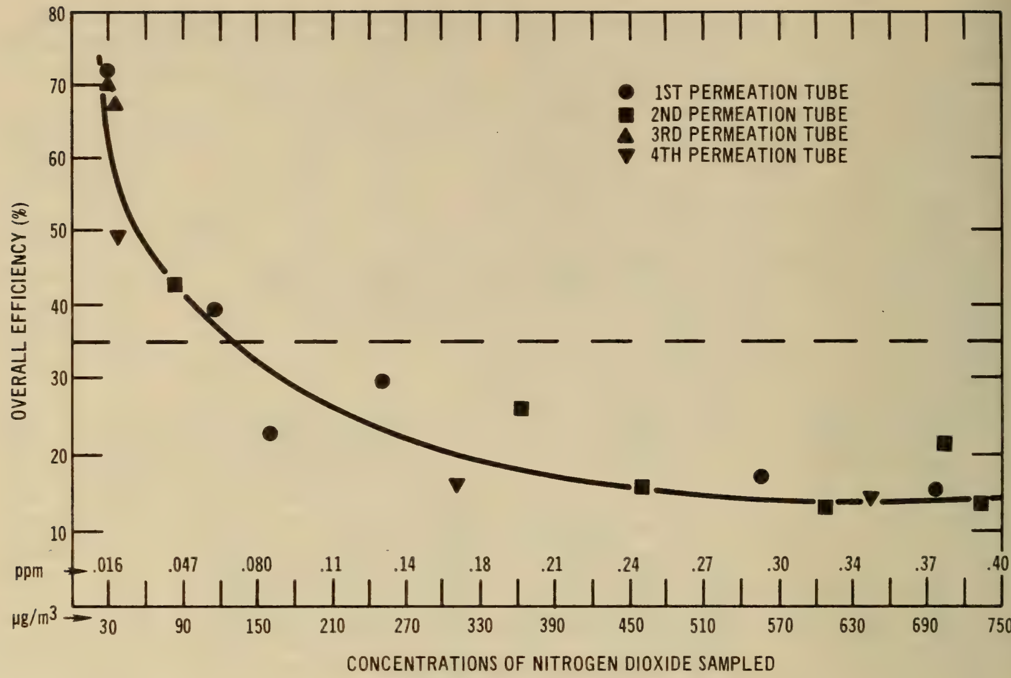

Fig. 1.—Response to the NO_2 reference method.

standard. Such sites were located in Los Angeles, Chicago, New York and Baltimore. A somewhat lower ratio of 1.6 between a slightly modified version of the Federal Reference Method and the arsenite method was obtained at 7 sites in Chattanooga, California and St. Louis by the same group of Federal investigators who conducted the Chattanooga School health studies of nitrogen dioxide 3 years prior to these recent aerometric studies. When the ratio between the continuous Saltzman and Federal Reference Methods (FRM) was compared, the Federal Reference Method was 2.6 times higher in the 6 continuous air monitoring stations which are part of the same operation that maintained the National Air Sampling Network. When similar data from 3 stations operated by the investigators conducting health studies were compared a ratio of only 1.4 was observed. Interestingly enough, almost the same difference in FRM to Saltzman ratios, 3.1 vs. 1.5, was noted when these 2 groups operated stations in close proximity in the same city. In the critical region for health effects, that is between 90 and 149 $\mu g/m^3$, the Federal Reference Method in the hands of the health investigators gave readings that were about 20% lower than the Saltzman method, whereas Federal Reference Method readings from the continuous air monitoring program were more than twice as high as Saltzman readings. Fortunately, it was also possible to compare Federal Reference Method readings made by the health investigators in Chattanooga with Saltzman readings made at a nearby site by the U.S. Army. Overall and within the critical concentration range, the Federal Reference Method to Saltzman ratios were intermediate, being higher than those observed by the health group and lower than those found in the continuous air monitoring program. These relationships, seen in Fig. 2, can be compared with what one would expect given the theoretical collection efficiency curve and assuming that there was no significant diurnal variation in nitrogen dioxide. This analysis explains why the Federal Reference Method appeared so

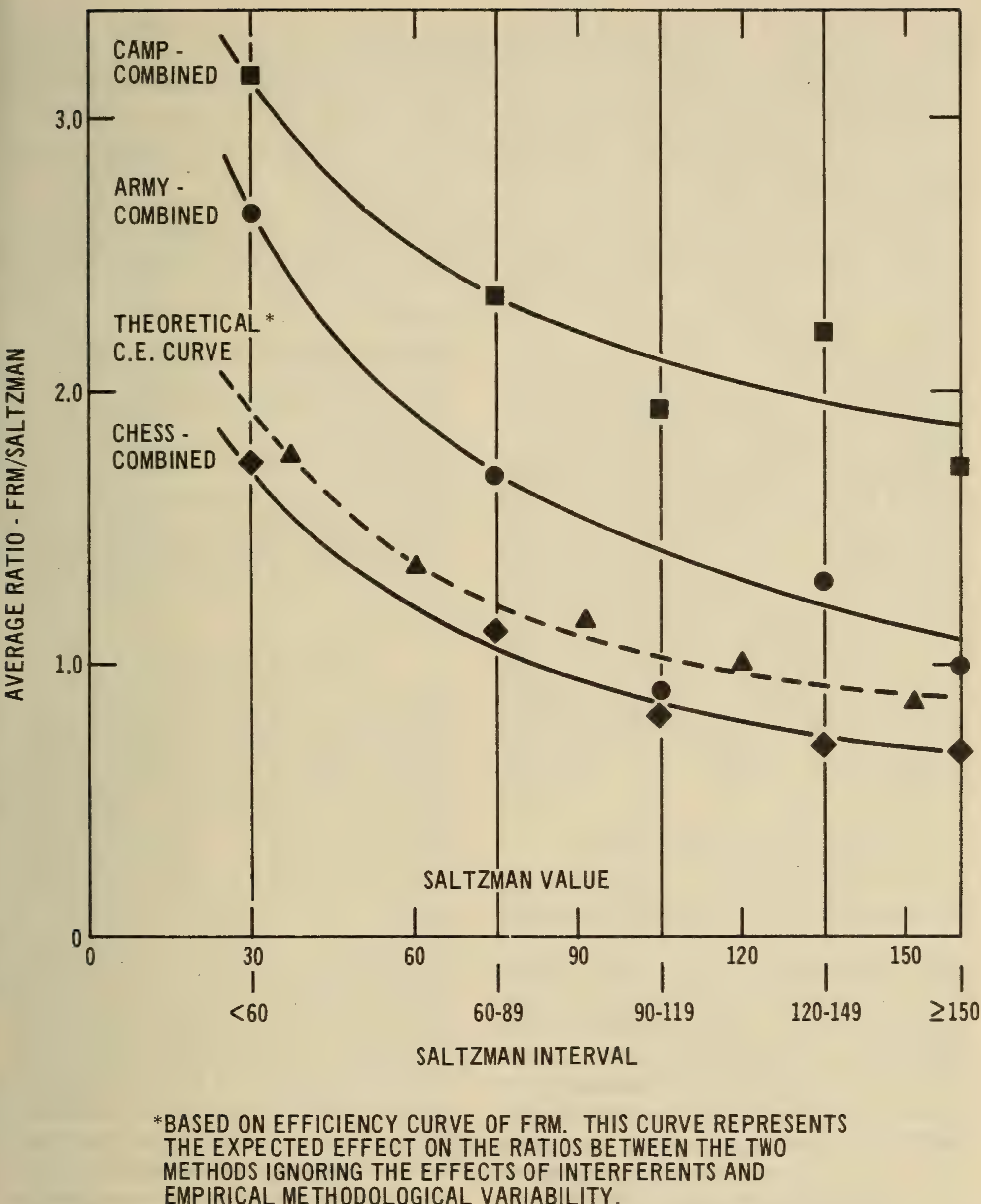

Fig. 2.—FRM FRM/Saltzman ratio vs. Saltzman interval.

much higher than the Saltzman method in cities participating in the continuous air monitoring program. More importantly, the analysis points out the need for a strong quality assurance program for air quality measurements. The foregoing analysis also helps explain why the Federal Reference Method happens to give a fair approximation of Saltzman measurements in Chattanooga during the Chattanooga health studies but a poor approximate elsewhere. A reanalysis of these health studies using only Saltzman measurements will be discussed later.

Another legitimate question is to ask

how well the 3 tentative candidate methods compare with one another. The continuous Saltzman method operated by the continuous air monitoring program (CAMP) compares fairly well with the arsenite method except when nitrogen dioxide concentrations are very low. In the former case the ratio between the 2 methods ranged between 0.8 and 1.2 with a correlation coefficient of greater than 0.8. The ratios were higher (1.2 to 1.5) and the correlation poorer when the Saltzman method was used by our health investigators. The continuous Saltzman and chemiluminescent methods were also compared and the health investigators seemed to get a better relationship with ratios of 1.1 to 1.4 than did the CAMP program ratios of 0.7 to 1.3. Unfortunately in this comparison the correlation coefficients were quite variable—0.3 to 0.8—for both groups. One can thus state little more than that the 3 methods seem roughly comparable in field settings and that the planned standardization and quality assurance programs are clearly needed.

The Case of the Oxidation Catalyst

One way to reduce the amounts of carbon monoxide and unburnt hydrocarbons emitted by current sparkfired internal combustion engines is to pass exhaust steam through an oxidation catalyst which converts these pollutants to harmless carbon dioxide and water. Work with oxidation catalysts began over a decade ago and this course was chosen by major U.S. auto manufacturers four years ago. Catalysts-equipped vehicles require low phosphorous, lead-free fuels because these substances adversely affect catalyst performance. Current legislation requires that automobile manufacturers develop control technology for reducing automotive emissions but the pathway and time frame for assuring that control devices pose no public health problem is less clear. At any rate, health problems could arise from undesirable thermal effects of improperly shielded catalytic converters, the emission of catalyst attrition products or the ability of catalysts to alter unregulated mobile source emissions. Catalysts are scheduled to be installed on 1975 model year vehicles. A vigorous research program is underway to assess what health trade-offs might be involved. Major certainties involve emission levels of catalyst attrition products and acid aerosols, dispersion of these pollutants, the magnitude of the resulting personal exposures and the expected adverse effects. Research programs in the Federal Government and in industry should avoid similiar problems in the future by making safety assurance an integral part of the research and development effort devoted to any control technology.

Summary and Conclusions

Protection of public health and existing legislative mandates require that automotive emissions of carbon monoxide, hydrocarbons, oxides of nitrogen and a number of currently unregulated emissions be reduced to acceptable levels. Scientific uncertainty makes the task extremely difficult and contributes to public acrimony. Unrestrained advocacy hampers efforts to reveal existing information, reduce uncertainty and avoid emerging problems. Despite these societal and other technical difficulties progress is being made in that air quality is beginning to improve. Case studies of nitrogen oxides and catalytic converters illustrate the interrelationship between major technical components required by a rational control effort. Shifting from the present "no-effect" threshold risk philosophy to a cost-benefit risk philosophy would only intensify the impact of technical uncertainties. The most rational approach is to unite and intensify governmental and private efforts to reduce bothersome scientific uncertainty to more acceptable levels.

Some Aspects of Determining New Motor Vehicle Engine Emission Levels

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Automobile manufacturers are certifying new motor vehicles and new motor vehicle engines in accordance with regulations established by the Environmental Protection Agency (EPA) for the control of air pollution. These EPA regulations are contained in Title 40 of the Code of Federal Regulations (CFR)—Protection of Environment, Part 85.

The emission standards set limits on exhaust emissions, evaporative emissions and crankcase emissions. For example, emission certification levels for 1973 and 1974 light duty vehicles for hydrocarbons, carbon monoxide, and oxides of nitrogen as measured by the constant volume sample-cold test procedure (CVS-C) were 3.4, 39.0, and 3.0 g/mi, respectively. In fuel evaporative emission tests, the hydrocarbons were not to exceed 2 grams and no crankcase emissions were permitted to be discharged into the ambient atmosphere from any new motor vehicle.

The certification procedure is described in 40 CFR, Part 85. It addresses such matters as application for certification, approval of procedure and equipment, required data, selection of test vehicles, vehicle and engine preparation, gasoline specifications, chassis dynamometer driving schedule, emissions sample procedures and equipment, information to be recorded, calculations of emissions, compliance with emissions standards, and testing by the EPA Administrator.

Of the guidelines provided in the certification procedure, this paper will focus on certification to exhaust emission standards.

Certification to Exhaust Emission Standards

Certification test vehicles designated in the regulations as durability data vehicles are driven, with all emission control systems installed and operating, for 50,000 miles or such lesser distance as the EPA Administrator may agree to as meeting the objectives of the test procedure. Emission tests are to be conducted on these vehicles after 4,000 miles of driving and at accumulated mileages that are multiples of 4,000 miles. (The mileage intervals increased to 5,000 miles for 1975 light duty vehicles.) Additionally, test vehicles designated as emission data vehicles are required to be driven 4,000 miles with all emission control systems installed and operating. Emission tests are to be conducted on emission data vehicles at zero miles and 4,000 miles. Fifty thousand-mile emission levels for each emission data vehicle are computed by multiplying the 4,000-mile exhaust emission test results by a factor. This multiplier is called the deterioration factor (DF), and is computed from the emissions data produced by the durability data vehicles. It is expressed as:

$$\mathrm{DF} = \frac{\text{exhaust emissions interpolated to 50,000 miles}}{\text{exhaust emissions interpolated to 4,000 miles}}$$

Values for the numerator and denominator of this ratio are required to be taken from a straight line, like the one shown in Fig. 1, where all applicable HC measurements made on a durability data vehicle are plotted as a function of the mileage on the system. It should

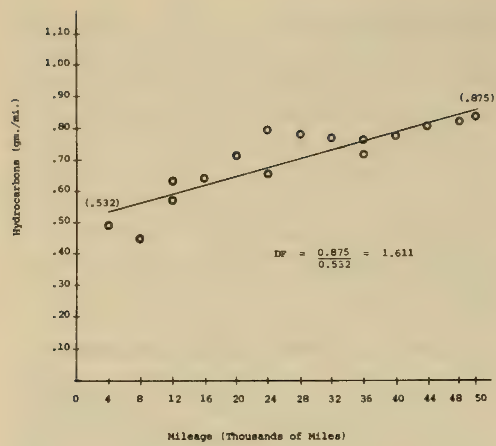

Fig. 1.—Graph for determining deterioration factor (Durability Test Vehicle No. 1012—for hydrocarbons).

be noted that HC measurements were made at 4,000 miles, mileages that are multiples of 4,000, and at mileages where scheduled major maintenance (e.g., tune-up point) of the durability vehicle took place. In the case of major maintenance both before and after maintenance tests are included.

The straight line fitted to the emission data is to be a least squares best fit straight line. The interpolated exhaust emissions that are required for determining the deterioration factor are defined as the 4,000-mile and 50,000-mile intercepts on this line. In Fig. 1, their values are 0.532 g/mi of HC and 0.875 g/mi of HC, respectively. Their ratio, 0.875/0.532, provides a DF value of 1.611 obtained from HC measurements on durability test vehicle No. 1012. As noted earlier, to determine compliance of an emission data vehicle (4,000-mile vehicle) of the same emission system combination, the 50,000-mile HC emission level is estimated by multiplying its 4,000-mile HC emission level by the DF, 1.611. This extrapolated emission value then must be below the applicable acceptance level.

In the event that a durability data vehicle is not tested to 50,000 miles (with the approval of the EPA Administrator), the data for mileages greater than that actually run are to be determined by extending the line of best fit established for the test data at lesser mileages.

Further, it should be noted that if a deterioration factor as determined by the aforementioned method is less than 1, then according to a rule stated in 40 CFR, Part 85, that deterioration factor shall be assumed to be one.

Separate emission deterioration factors are to be determined from the emission results of the durability data vehicles for each emission system combination. Also, an individual deterioration factor is to be established for exhaust hydrocarbons, exhaust carbon monoxide, and exhaust oxides of nitrogen.

When the procedures discussed above are followed. the practice gives rise to some interesting questions. These questions are examined in some detail in the next section of this paper.

Discussion of the Procedure for Calculating and Applying the DF

As shown in the preceding section, a deterioration factor for an emission system combination is defined as the ratio of ordinate values of two special points on a line that is a least squares linear fit of emission data collected over 50,000 miles of emission testing on a durability data vehicle.

The deterioration factor developed by 50,000-mile testing of a representative vehicle is then used as a predictor of emission durability characteristics of similar vehicles which are tested only through 4,000 miles. The 4,000-mile levels are projected to 50,000 miles by application of the appropriate deterioration factor.

The purpose of the deterioration factor is to provide a means for predicting emission compliance at 50,000 miles without actually testing all certification vehicles, as selected by the EPA, over the entire 50,000-mile durability test schedule. Thus, cars are tested at 4,000 miles and emission compliance is determined by projecting these 4,000-mile emission levels to 50,000 miles by means of the vehicle emissions system "predictor" (i.e., the applicable deterioration factor).

A curious aspect of the procedure for

determining the deterioration factor from a least squares best fit line is that the line is not used as a regression line in the usual sense. Ordinarily, a regression line used for making predictions is based on certain underlying assumptions about past performance. In this instance it is assumed that the deterioration factor represents deterioration of the emissions system between 4,000 miles and 50,000 miles of operation. However, the deterioration in emission levels that occurs between 4,000 miles and 50,000 miles can be viewed as the difference between the 4,000-mile and the 50,000-mile intercepts on the least squares best fit line (as is the case for evaporative emissions and heavy duty truck exhaust emissions). Such depreciation is not represented by a deterioration factor expressed as a ratio.

As mentioned above, the current method of determining the deterioration factor for exhaust emissions from light duty vehicles is based on a least squares best fit line. Once this line is established, only the ratio of the 50,000-mile to the 4,000-mile intercepts is used for predictive purposes. This approach tends to ignore the significance of the slope of the least squares best fit line. This point can be best exemplified by examining Figs. 2-A and 2-B.

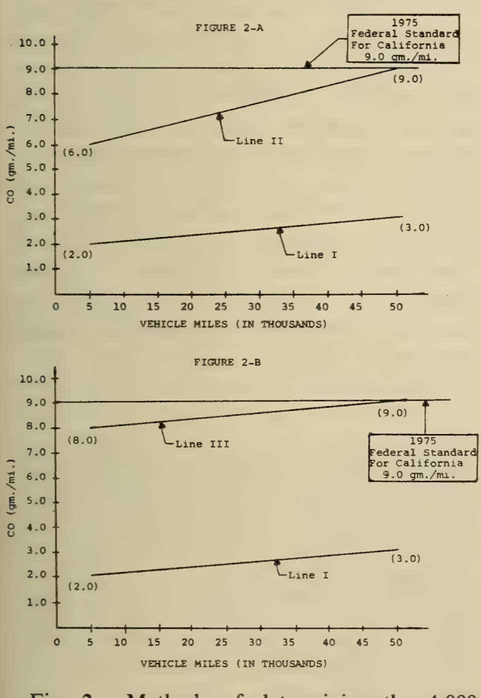

Fig. 2.—Methods of determining the 4,000-mile emission "bogey" level for exhaust emissions from light-duty vehicles.

Let us assume that a 1975 model 50 state certification vehicle was run to 50,000 miles and exhibits emission performance for carbon monoxide as shown by Line I in Fig. 2-A. The 4,000-mile and 50,000-mile intercepts are 2.0 g/mi and 3.0 g/mi respectively, with a resultant DF (ratio) of 1.50. Applying this DF to the emission acceptance level, a maximum allowable 4,000-mile emission level or "bogey" can be generated, which all 4,000-mile emission data vehicles must be less than or equal to. In this example, the CO "bogey" is 6.0 g/mi.

$$\text{4,000-mile bogey} = \frac{\text{50,000 mile standard}}{\text{DF}}$$

$$\text{4,000-mile bogey} = \frac{9.0}{1.5}\ \text{g/mi} = 6.0\ \text{g/mi}$$

Reconstructing an emission performance line based on the 9.0 g/mi acceptance level and 6.0 g/mi "bogey" yields Line II in Fig. 2-A. From a practical engineering standpoint a comparison of Lines I and II of Fig. 2-A indicate that two different deterioration rates exist. However, as defined in 40 CFR, Part 85, the deteriorations have the same representation; i.e., the deterioration factors (when expressed as a ratio) are identical.

Line III on Fig. 2-B can be regarded as more representative of actual emission depreciation on the durability vehicle because it has the identical emission deterioration rate (slope) as the 50,000-mile vehicle. Thus, in this context, a "bogey" of 8.0 g/mi is more appropriate. Interestingly enough, Line I and Line III, with identical deterioration rates, would obviously have significantly different DF's when calculated by the ratio method (1.50 vs 1.125, respectively).

An incongruous feature of the application of the deterioration factor is that the deterioration factor, from the 50,000-mile durability vehicle, can only be used to determine emission system compliance if both the 4,000-mile and 50,000-mile intercepts are below the acceptance levels. Based on earlier discussions of the purpose of the DF (i.e., to project 4,000-mile emission levels to 50,000 miles) and the fact that emission system compliance to standards is predicated on the 4,000-mile emission data vehicle's projected 50,000-mile emission levels being below the standard, it would appear that such a constraint on the DF is rather severe. For, as previously stated, the DF is nothing more than a predictor of emission system depreciation and, as such, should be a valid indicator regardless of the actual emission levels of the durability vehicle selected to represent the emission system.

How good, then, is the DF expressed as a ratio and the 50,000-mile emission projection resulting from its use? Would not the difference between the 50,000-mile intercept and the 4,000-mile intercept of the straight line be a better measure of total vehicle emissions depreciation?

The DF, which is a variable in a statistical sense, is used as shown earlier for setting manufacturer's development objectives. Knowing more about the statistical properties of the DF is essential to finding answers to some of the problems raised in the next section of this paper.

Factors Associated with Setting In-House Emission Development Objectives

Given a 50,000-mile emission level that a certification vehicle should not exceed (acceptance level), a manufacturer may desire to set an in-house development objective for a 4,000-mile emission data vehicle so that he can be reasonably assured that any individual certification vehicle within the emission system will qualify.

To address this problem, a statistician needs to know how measured and calculated emission values tend to be distributed; that is to say, how they tend to vary. He needs to understand, too, to what extent calculated DF's can be expected to vary. Unless the nature of these distributions is known, it is difficult to establish the probabilities implied in the preceding paragraph, and it becomes necessary to consider other statistical approaches to setting emission developmental objectives.

For resolving some of the earlier-mentioned problems, Monte Carlo simulation is a useful approach. This is a methodology that usually requires the assistance of a computer for constructing and sampling distributions from which estimates of the desired probabilities can be extracted. This approach is being used in some quarters.

Summary

Certification criteria make use of a factor that is defined to reflect deterioration of the emissions control system up through 50,000 miles of usage. This paper explains how the deterioration factor is used for projecting a 50,000-mile emission level from an observed 4,000-mile emission level. Recognition was given to the fact that the statistical properties of most of the observed and calculated variables need to be better understood. No attempt was made to provide answers to the statistical questions that were raised in the discussion. Useful approaches to some of the statistical problems were provided in hope that more attention would be given to a statistical base for evaluating emission-related systems.

Acknowledgments

This paper could not have been written without the assistance of Mr. D. N. Hwang, who first brought some of the statistical problems to my attention, and to others in Ford Motor Company who have given these problems considerable attention and who were generous in sharing their views on the problems and their possible resolution. The help of these gentlemen and others from Ford Motor Company is appreciated.

Air Pollutants—Safe Concentrations? *Panel Discussion*

Chairman: **Dr. John D. Hromi,** *Ford Motor Co.*

Panelists: **Dr. William H. Kirchoff,** *National Bureau of Standards*
Dr. Vaun Newill, *Environmental Protection Agency*
Dr. Nozer D. Singpurwalla, *George Washington University*

DR. HROMI—The theme of this conference is "Statistics and the Environment." We have heard this morning about some problems that suggest a statistical approach to a solution. We hope that, interspersed among the questions today, will be those that pertain to the use of statistics. I don't think that it is intended today that statistics will be presented as a panacea for all problems that confront us today. After Dr. Finklea's presentation this morning we all appreciate the complexity of the control of air pollution. I think, however, it is necessary to establish a perspective that puts statistics in a role of helping to solve some of the related problems. I now ask for questions.

DAVID SALSBURG (Kaiser)—I have a few comments or questions, first of all for Dr. Newill. Has anyone considered or organized planned experiments on a national scale similar to the way advertising news is evaluated? Something like this: You would pair off relatively small communities and take some sort of stratified sample of cars that represented something better than 50%, apply to each community a different method of emission control, then for six months take measurements on ambient air values on short-term health characteristics just to give some kind of well planned experimental design. Secondly, Dr. Hromi. I was impressed by the point you made. I took a quick look at the variance of this ratio. You might point out to the Federal authorities that by picking the two end points of the line, they have exaggerated the variances as much as possible, because the variance of the predicted value of y is of course a function of the square of the distance from the x bar to the value.

DR. NEWILL—As far as I am aware we have not considered any planned experiments on a national scale. There are several reasons for this. One is that resources are scarce, and an effort to do this kind of thing would be extremely expensive. The second is that we would be using the general public as a testing system, to which there is a great deal of aversion. In human experimentation in this country, one must have informed consent for participation. This would make it rather difficult, since whether you think you are only looking at the different strategies or not, you are in fact involving the population.

DR. HROMI—I like the question that was raised from the standpoint that any experimental program, no matter how complex, requires some planning and forethought. On a number of occasions Dr. Finklea mentioned the desirability of accumulating certain kinds of data. This is basic to other research problems, too. We need to think through an experiment before we conduct one. We should decide what kind of data to collect and how much data to gather and what to do with them *a priori* to actual experimentation.

DR. JOHN GOLDSMITH—The experimental data to which the previous questioner addressed himself are accessible in the sense that epidemiologists have a chance to observe results of natural or technological perturbations. That applies, for example, to the requirement for certain types of motor vehicle

exhaust control in California, at an earlier date than in the rest of the country. It is conceivable, that additional data sets can be analyzed especially with the kind of approach which Prof. Box earlier presented to this Symposium. One could detect the contribution of various types of control systems in several types of communities, but the control of all other measured variables may by no means be sufficient. There is an increase in the proportion of motor vehicles which use fuels of different class, in relation to the requirements of the engine as determined by the manufacturer. For example, the sulfur content of fuel sold in the southern California basin is higher than in other parts of the country. Climatic as well as meterologic conditions may affect the resulting pollution levels. Nevertheless, we do know a lot about how some of these variables behave.

I might add that the impending use of catalytic exhaust control systems in California provides a new and perhaps even more important opportunity for obtaining such data.

Dr. Hromi, it seems to me that the approach and the questions you raised about the deterioration factor are a matter of some consequence. Of course there are mathematical relationships between the variance of a ratio and the variance of its numerator and denominator. However, one must have all of the data relevant to the numerator and denominator, and as one who has only occasionally looked at emission data, I find some peculiar truncations. Unless all of the data obtained in such a series are available, the estimates of variances that have been made, at least in the open literature, are rather peculiar. The truncations that I have observed show a clustering of values just below the accepted emission standards set by the California Air Resources Board. Would you like to comment?

DR. HROMI—Your comment on truncation is an interesting one. It leads me to mention truncation in another sense:

When a deterioration factor is calculated according to the Code of Federal Regulations and turns out to be less than one, we then must assign to it a value of one. A truncated deterioration factor distribution results.

My involvement with this problem is rather recent and my background is unlike that which you appear to have, yet I think it is easy to understand that we need to know more about the quantities that we are asked to analyze and interpret.

DR. HAROLD PECK (Merck)—Speaking as a consumer, it is desirable to have those ideal conditions where you have no emissions whatever as opposed to the situation in which there is no control. Obviously, we are going to have something in between complete control and no control. These controls cost money in the terms of original cost, in terms of fuel economy, in terms of maintenance and probably replacement. When is the public going to rebel because of excessive cost? There is already a lot of concern about the difficulty of starting cars, keeping them running until they get warmed up, and the increased use of fuel, particularly in the fuel shortage. How can we calculate the point at which the public will break?

DR. NEWILL—I don't know. Certainly public opinion is an extremely important thing in determining research priorities both in terms of what is done in the agency and the constraints that are going to be placed on regulation. There are many social variables that are just not being looked at in terms of any of these things. I can only say that this is another one of the areas we should be taking into consideration. In many ways the energy crisis has been very helpful because people have begun to discover what part of their transportation is essential and what isn't. I think both the public and the people within some of the agencies are better able to look at these problems than they were a year ago.

DR. KIRCHOFF—I have a few comments. First of all, concerning Dr. Peck's question about public tolerance and just how much the public can stand, it doesn't appear to be in the Clean Air Act at all that EPA is to worry about the economics of environmental control—it is simply to get the air as clean as possible. If the public really demands a different approach, then it will probably have to go through the Federal legislature. Dr. Hromi, I actually wanted to talk to you a little bit concerning your presentation. I have a couple of questions and, before you respond, a statement about statistics. The questions are: How many automobiles of a given class were used in the durability tests? How many automobiles were used as the certification vehicles? The comment is about statistics because the argument you raised this morning was clearly statistical in nature. You argued that one way to look at a certain class of numbers is better than another way. Eventually arguments such as these will be presented to legislators or policy makers and may very well exasperate them. If I were a member of a Senate Subcommittee, I might say "Well heck, you are from the automobile company and the reason you choose one approach over the other is because you can get by with a higher level in your certification vehicles." As a policy maker, I would be hard pressed to make judgments based on statistics. Now concerning the argument you presented, you were in reality making some assumptions about the nature of deterioration—that deterioration is something that occurs in an absolute rather than in a proportional sense. It was thus not really a statistical argument at all, but rather an argument about the nature of deterioration. You could perhaps determine the nature of deterioration if you took several thousand cars out and ran them around the track for 50,000 miles, but without such an experiment, your argument is only conjecture.

DR. HROMI—I think your question is a very good one. They are questions that certainly are of untold concern to both industry and the regulators. We in industry do not choose an approach. The approach that I defined and questioned is in the Code of Federal Regulations, so we have no choice in the approach. The Code of Federal Regulations, Title 40, Part 85, contains a procedure that specifies the selection of cars by the EPA based on sales volume. In certain makes and models in which sales volume is not appreciable there is a minimal number. I don't know what our recent numbers of test cars are, but there could be as few as one per engine family—at the discretion of the EPA.

DR. KIRCHOFF—What is the maximum number that could be tested?

DR. HROMI—I have no idea what the maximum number could be, but it certainly is not a thousand.

DR. HENDERSON (Olin)—Mention was made of human experimentation, and Dr. Finklea indicated some questions about the catalytic converters that will be mandatory in 1975. It would appear to me that we are making the total US population an experimental group for a system that is possibly questionable. I would like some comment on that.

UNIDENT.—California is the only place they are going to be used in 1975.

UNIDENT.—So California then becomes the test population? Has everybody in California given their informed consent for that experiment?

UNIDENT.—In California catalysts are being introduced on a very limited scale. How much of the car population is actually changed over in a course of one year and how much adverse health effect will come from that limited edition exposed to the population? By actually introducing it in such a limited fashion you will have the opportunity

to monitor and discover whether or not our speculations are valid. The actual quantity of sulphur in gasoline that will be converted to sulphate is not very large. The problem lies in the fact that it will be concentrated close to major freeways, so that there may be much higher exposure levels than we really want. In estimating those, we have used only dispersion models. Any of you who have been part of that process know about the tremendous fight over whether the models we have used are proper or not. We can detect many of the problems only by making some kind of limited introduction into the population and observing for the suspected risks.

DR. HROMI—You indicated that some results take a long time to acquire. Dr. Singpurwalla, would you care to address that question from the standpoint of a reliability engineer and statistician who is studying accelerated data collection?

DR. SINGPURWALLA—Yes, I would. The problem that I have been working on is the analysis of accelerated life tests—that is, data collected under accelerated environments. I suspect that this is a very nice analogy wherein similar techniques could be used in environmental studies, whether they involve human populations or automobile populations. There are methods by which such failures can be analyzed based on physical hypotheses or biological hypotheses about certain failure mechanisms and those techniques could essentially be transferred to whatever extent they are feasible into the problem of this particular nature. Along those lines, Dr. Hromi, your first view graph contained a straight line for hydrocarbons on the vertical axis and mileage on the horizontal axis. Without running a regression I notice that rather than being a straight line, might it be two segmented lines?

DR. HROMI—Your point is well taken and it's one that we ourselves question. Yet I want to stress that at the moment this is the direction that was provided by the Code of Federal Regulations, and this is the way we must respond. We must draw least squares best fit lines over those points. Whether the points suggest something else is another matter.

DR. DOMEY (U. Texas)—This is far too peaceful a conference. Dr. Hromi, let me ask you a question. In 1957 the city of Seattle was the scene of a conference that had to do with the problem of atmospheric contamination by vehicles. At that time we were assured by representatives of the industry that they were hard at work on this problem. In the meantime the DuPont Corporation had outfitted several vehicles with the complex equipment capable of continuous monitoring of atmospheric contaminants. Registers in four colors for example, on a continuously-rotating paper were presented. First of all, we are here because of a mutual concern over this problem. I wish to defend my friends in Government, though I do not consider them necessarily my close friends. At least they have generated a target at which industry must direct its criticism. It matters not whether this line slopes this much or that much at the moment. That is a trivial point. My question is to industry, which has had some 15 years to collect the data. Where are the basic data?

DR. HROMI—I wish I were able to answer that question. I think I have indicated that I am not involved in emission work on a continuing basis. My occasional involvement is that of a statistical consultant. I have no idea what kinds of records were kept over the last 15 years. Dr. Domey, I would like very much, though, to take your question back. So, after the session let's formulate the question you would like to have answered and I'll try to get an answer. I'll see you after the session.

DR. BOX (U. Wis.)—I also felt very sympathetic with the last speaker. The thing that I wonder about is the provision for feedback. The Ford Motor Co. has a great deal of money and a lot of expert statisticians and they presumably have recommendations as to how this analysis ought to be made. You did hint that there is some kind of conversation going on. Is there some mechanism whereby there can be some negotiation and things can be changed?

DR. HROMI—Yes. However, let me go back a moment and say that it certainly was not my intent to make anybody else look bad. I try to focus on some problems that exist because of the state that we are in at the moment. When I started to write the paper I was privy to some information that existed in the '72 and '73 version of the Code of Federal Regulations. I was reminded recently that there have been many changes in those regulations that have occurred over the last three or four years as a result of dialogue, some formal and some informal, with people in the administration. As far as I know, there isn't any routine mechanism for providing feedback.

DR. BOX—There's nothing like a hearing where people can just come and give evidence?

DR. HROMI—I know of none.

DR. BOX—Just one other comment on the question of ethics—of the public being experimented on and so on. It seems to me that we are always in this situation to some extent. In the testing of cancer drugs and so forth—first, there are benefits; second, there are possible dangers; and third, have we done everything we can do before we get to this point? It is clear there is a great deal of information already potentially available possibly which is viable but not yet fully exploited. For example, there is a 17-year record of hourly measurements of several pollutants at a number of different locations in Los Angeles County. We were asked at Wisconsin about 18 months ago to (a) try to get the data out so that it was available for everyone, and (b) try to discover its significance. This meant that we had to invent some statistical methods. We've been working pretty hard on that. I imagine there are other sets of data as well. The fact is, in Los Angeles they have introduced a number of new regulations from time to time. I suspect that some of these regulations have made no difference whatsoever. As far as I know they have not repealed those laws. But there are some effective laws, and the effects have not been exactly what one would have expected. Others of us can learn from this experience.

The American public is somewhat spoiled and I think it may be a very good thing if it is in for a period of deprivation. It may set some values straight. Americans live in a poor world and are going to go the way of other aristocrats unless something is done about it. I could manage with less money than I do. I don't really need two cars. I would be a lot better off if I cycled to work. Having an energy crisis, and reorienting our priorities, and the fact we are not going to be fooled into buying big cars we don't need and very often using drugs we don't need either is going to be a very good thing. And I remember that in England during WW II when we had fair rationing, we got 10-penny worth of meat but we knew that nobody else was getting more, and we had a pretty good time. People had a purpose then, and the war years held some of the happiest times I remember. Shortages don't necessarily mean that things are going to be black—they are perhaps just going to be more interesting.

DR. HROMI—Something else occurred to me this morning after George made a point about having a formal channel for a continuing dialogue. Fred Leone, one of the organizers of this Conference, talked with me. Fred said that one of the purposes of this Con-

ference is to try, at least on an informal basis, to establish more healthy dialogue between the kinds of people in this room. But then he asked the question, "What next?" I think that bears on what you have in mind, George. A result of this symposium should be a continuing kind of forum such that, even though my remarks might have sounded like they were intended to be maliciously critical, can be regarded as constructively critical. We need a forum where we can put these problems out on the table.—where we can attempt to solve some of these problems with the kind of knowledge we already have, or define needs for new knowledge, as you indicated was necessary in the California study.

DR. MARCUS (U. Md.)—Dr. Kirchhoff, you talked about the possible extremely serious potential health effects of pollutants from the internal combustion engine. We have talked about one control strategy, which consists of putting some sort of Rube Goldberg device on the tailpipe to be obscurely evaluated, argued about, and probably disconnected in appreciable numbers. There are other alternative transportation control strategies. The core of the problem is that about 40% of the workday motor vehicle trips in urban areas are made during the four peak hours with an average ridership of about 1.3 people per car. It seems to me it should be the purpose of the Department of Transportation as much as EPA, to do something about the control strategies in that area. Are the air pollution emission consequences of some of the strategies being monitored or assessed and to what extent? Are they being developed?

DR. KIRCHOFF—That is a double barreled question, part of which I cannot answer. As you know, car pooling is one measure to reduce peak transportation demand. Mass transit and the incentives which have been placed on the expansion of mass transit are measures which the Department of Transportation has taken seriously, not so much to reduce pollution but to conserve energy. I think Dr. Newill can probably tell you more about monitoring of emissions. Traffic volume will surely be monitored very carefully and various areas will be combining these data with emission data to find out exactly what the effects are.

DR. NEWILL—The implementation plans that are required for an area to actually meet the ambient air quality standards requires monitoring. A great deal of monitoring is being done, much more than a few years ago, so that there will be data available. The intimation has been that the energy crisis hasn't gone for a long enough period of time for us to actually have those data. There are several reasons for that. One reason is that the data probably still reside in the places where they're originally gathered, not yet having reached the offices where they will be evaluated. I'm sure that a great deal of monitoring is going on for most of the pollutants we are discussing here.

The EPA has recognized that there hasn't been as good a forum for the scientific community as there should be. Industry, more than the scientific community in general, has availed itself of a direct opportunity by asking for meetings and presenting their point of view. They haven't always had a time reaching the people within the agency as they probably should have, and this was part of the impetus behind the establishment of a Science Advisory Board within the Agency, the approval for which was published in the Federal Register on January 18. The Board is being assembled at the present time. I think this will be a mechanism whereby the scientists can, in fact, communicate with the agency in a fashion differently than they did before.

I have talked to Fred Leone about the possibility that this association should ask the Agency to place some statistical talent on that particular Board—

someone who can reflect to the administration the need for this kind of thing. We are talking about all the short-term solutions, but they will take a very long time to come—generations. One of the biggest things we have to change are people. All of the problems we are talking about here should be taken particularly seriously by those in academic communities. Here is where they can think unencumbered about these long-term problems and can begin to train a new generation of people to handle them in a different fashion than in the past. As with so many other things, time will put the problem into perspective. These short-term effects are certainly not going to be disastrous.

DR. HROMI—You made a comment that reminded me about the mechanisms for establishing a dialogue between the regulator and the regulated. I don't know what your agency's mechanism is. I am somewhat more familiar with the opportunity to discuss proposed rules and regulations before they become law in the National Highway Traffic Safety Administration in DOT. Notices of safety standards are posted in the Federal Register and the whole world has an opportunity to respond before the proposal becomes a law.

DR. KIRCHOFF—There is always a problem in communication between regulator and regulatee in that the relationship is primarily an adversary one. Perhaps this is because the government is run by lawyers. I don't think scientists are comfortable with such an adversary relationship. We would rather approach problems from the standpoint of ascertaining the truth of a particular idea rather than to present arguments supporting a particular point of view and to subject these arguments to judgment. Unfortunately, this adversary relationship persists in the public hearings related to environmental legislation.

UNIDENT.—I think it is time to express a little bit of the 30-year experience of the AEC with some of these problems. Relative to the public having a chance to have its opinion brought to bear, the Agency does respond to the legislative and executive branches of the government—it is really through them that the public has its first avenue of expression. Public hearings have become standard, and adversary relationships are going on constantly. However, getting into the scientific arena does not eliminate the adversary nature one bit. In fact, the situation is made even more difficult. Our most trying adversaries in the atomic energy business are some of the world's best scientists. This is an intellectual challenge that makes it good fun. However, the suggestion that the informed public should have a plebiscite on the types of regulations that are made leads to "every man for himself" in the case of unrestrained advocacy. In this regard I think that the AEC and EPA are sometimes in confrontation. In their defense, we have to respond to public needs and interest within the limits of enabling legislation, with full opportunity for the public to have redress through the judicial process. It is a long, tedious procedure but it is available and it certainly is better than the plebiscite approach to the problem.

We have been challenged lately on the possibility that radiation standards are not set for susceptible groups of the population. This is true—radiation standards essentially define the standard man. The evidence is considerably stronger now that there are susceptible sub-groups with regard to air pollution problems. Dr. Newill, how do you rationalize the problem of recognized susceptible subgroups in regard to setting risks?

DR. NEWILL—We are having a great deal of difficulty with this problem. At the present time we interpret the Clean Air Act to mean that you want to protect the people who are particularly susceptible to respiratory problems against an increase in the number of symptoms they have. In the

air pollution area I think we are doing a very good job. Other areas are more difficult. It will be some time before we have the same philosophical framework for our regulations and every media and categorical program. One of the reasons for this is that we operate under different legislative mandates in these different areas. A good example was that Mr. Ruckelshaus' statement about the reduction in the amount of traffic to meet the clean air standards in the Los Angeles area were really borne out of frustration that he had no flexibility in the way he could achieve them except to delay things for a year or so. He had to bring home to the people what the consequences were, and you will have to admit it did start a dialogue.

UNIDENT.—I am a statistician with the EPA. One must look at the purpose of the deterioration factor rather than worry about the specifics of it. The idea is to control the total emission over time of vehicles to set some sort of method of estimating what total emission true time will be. It is immaterial whether a least square fit is appropriate, or whether a normal distribution or a segmented function might be better. Rather, is this the best way of establishing a method for controlling total emission? That is the only criterion that should be used for judging these functions.

DR. KENNETH BUSCH (NIOSH) —Whether a ratio or a difference is appropriate between the 4,000-mile and the 50,000-mile values is one matter which can be determined by testing real vehicles and taking more data—it is an engineering problem that should be solved. The reason for using more multiple points on a fitted model is to smooth out the data and gain the advantage of precision from the additional monitoring data. You are predicting, from results of the test vehicle at 4,000 miles, a value which it would attain at 50,000 miles. Assuming that the difference model is appropriate, the prediction would have a confidence interval centered around the predicted value, so that 50% of the time the true value would be above and 50% of the time below the prediction. This means we are running a 50-50 risk of being higher or lower than the intended standards. Should we not use the upper confidence limit as a point of comparison, rather than predicted value?

DR. SINGPURWALLA—We have been confusing technical and non-technical issues. One individual says that it's not important whether it is a straight line or a normal distribution or a segment. Someone else agrees with him but says it should be looked at as a confidence level limit—we are getting into technical issues. I don't know what the answer to that is, but if we have the technical skills and the abilities to look at a problem as precisely as we can, why should we not look at it that way? Therefore, I challenge the statement that it is something gross that we should look at. . . .

UNIDENT.—I don't think it's gross. Let's answer the real question—not to get the best fit we can but to do the best job of answering the real question. These are two different things in this case.

DR. SINGPURWALLA—But would we be able to answer it better if we look at it properly?

UNIDENT.—Properly, yes, but properly may not be a least square sample.

DR. SINGPURWALLA—Oh, I agree. But as Dr. Schneiderman said yesterday just be sure you're asking the right question when you answer it better.

DR. JAMES TAYLOR—As an economist who is interested in fuel and energy, I would like to raise the point of gasses from high stacks, in electrical power plants for example. This is a problem that involves billions of dollars

of expense currently to the American public in capital investment and in expenditures for low sulphur fuels. Take the case of a large coal-burning electric power plant in southern Nevada, completed in 1971. To conform to the country air-pollution regulations, they say they must spend $100 million, provided an economical method can be devised for removing sulphur and other noxious gases from the emissions. The concept of the ambient air stream being affected by this kind of situation has been publicly challenged by Dr. Philip Abelson, President of the Carnegie Institution, in his annual report last year. He points out that more sulphur is put into the ambient air stream every day by nature than by man. It seems almost impossible to achieve the President's goal of self-sufficiency in fuel and energy production in the United States in the next 5–15 years unless large particles of coal with a good deal of sulfur content are burned. If we are to avoid that, we must spend a perfectly prodigious amount. I have heard nothing about this major problem in air pollution control, or anything about water control.

DR. NEWILL—One of the things that worries me is what the use of coal will to do to the environment *per se*. It is true that taking sulphur out of coal costs money, but many of the new processes such as coal gasification and liquefication will result in a much cleaner fuel. I wish that the wisdom to invest more money in those processes had resulted in some earlier budgets that would have allowed us to have the technologicals now. However, we weren't that wise, and we probably will suffer from some increase in adverse health effects. That nature puts more sulphur into the atmosphere than does industry doesn't concern me in the least. What does concern me is its effect on people. Industrial sulfur in the atmosphere is a distributional problem. We must determine how much risk people are willing to tolerate from their exposure. This has nothing to do with the energy crisis except that it might increase their tolerance a little bit. It doesn't mean that we should give up the idea of protecting people from these pollutants. Certainly we have to have short-range solutions to the problem, but the long-range goals should not be changed by the energy crisis. I don't worry very much about electrical power plants because the total amount of money being spent is nothing tremendous.

DR. HOMAN (Nat. Cancer Inst.)—I am a toxicologist. Some concern has been expressed by handling deterioration factors of less than one. A valid deterioration factor of less than one tends to imply that pollution controls, like good wine, improve with age. If you reject this contention, what is suggested with respect to data that produced such a number?

DR. GOLDSMITH—I would like to introduce a fairly important statistical problem—that of available data sets on air quality—which I think would yield useful results with some additional attention. I refer to the requirement under the regulations of EPA for establishing emergency plans. In California we are expected to notify people so they can take protective measures when we think something unusual may occur, such as high levels of air pollution over a specific period of time or at a specific location. Very often, available monitoring station data cover past periods. We currently face two classes of problems of a statistical nature for which I think some practical statistical applications would help us a great deal. The first problem pertains to a systematic use of sampling strategy to determine how well monitoring is located to measure what people are breathing. We have no reason to assume *a priori* that a given monitoring station is sensing the same air that is breathed by a given population, yet we have every reason to determine how these two are related. Collectively I think we who are espe-

cially concerned with health have been somewhat negligent because if we had been a little more articulate, perhaps it wouldn't have taken so long to get our point across to those who operate the monitoring programs. At present we don't know what monitoring stations represent in terms of area or population exposure. The second problem has to do with monitoring system data now available which will help us predict within certain probability limits how much exposure will exist at some time in the near future. For example, can we predict at 6 AM that it would be better to carpool than to drive one's individual car? While there is a good deal of discussion about car pooling, nothing is being done to facilitate it. There is no arrangement for providing gasoline; the very real economic incentives are very poorly documented; and there are usually no facilities for gathering people who want to ride in the same direction, although there are boards in which people are supposed to put cards. It's very difficult for car poolers to foregather in some windswept corner. Therefore, I ask the panel to suggest some practical way to solve these two statistical problems.

UNIDENT.—Regarding the car pooling situation, it is true that you see boards, but if you don't have a radio, you don't hear station WTOP promoting the car pool. Various industries are computerizing car pools, and a number of communities and industries throughout the country are using these computerized programs for car pool matching with various incentives. I believe Minnesota Mining has bought a number of 12-passenger minibuses for employee car pools. We would like to see ridership increase here in Washington from its present 1.6 to about double that. We feel that this would drop our peak hour concentrations 20% or better. Car and bus pooling has been very much in our minds and we are doing what we can about it, but it's awfully hard to wean the American from his personal transportation. I've been told that if gasoline goes to a dollar a gallon we'll have no problem except malnutrition, because some people will pay a dollar a gallon for gasoline and eat fried potatoes from then on and not get out of their car.

DR. KIRCHOFF—I wanted to make a comment concerning Dr. Finklea's mention of the EPA's problems with the NO_2 measurement techniques. Because of the unreliability of the EPA Reference Method for the determination of NO_2 a great deal of important data may have been irrevocably lost. A unique situation existed in Chattanooga in that a TNT plant was a prominent source of NO_2 in a rather local area. Health studies were made of people who were exposed to the NO_2 and people who weren't. These health studies, which relied on the NO_2 measurements for the determination of exposure levels, were critical in the setting of national primary standards and automotive emissions standards for NO_2. Well, the war in Vietnam is over and the TNT plant has closed and repetition of the study is no longer possible. A detailed description of the effect of the discovery of the unreliability of the NO_2 measurement method on the National Air Quality Standards and on the automotive emission standards appears in the June 8, 1973, Federal Register. A large amount of data is presented and I invite the statisticians in the audience here today to take a look at it. If anything, it should convince you of the need for a sound statistical and scientific basis for environmental decision. Information such as this is in the public domain whether published in the Federal Register or available from EPA under the Freedom of Information Act. Go look at it and work with it!

DR. LEONE—You just hit a sensitive nerve when you said that the information is there—go look at it. I don't think that is really what we want. Rather, let's get information together, talk about it together, plan the way we get it, and go ahead. We are trying to

agree to talk before the decisions are made—together we will talk about the risks, about how we get the data, about whether the data is meaningful, and about potential conclusions relative to the type of data we get. Not communicating is the thing we have to overcome.

DR. KASTENBAUM—The following quotations are from the book, "Geography", by Henrik William van Loon: "We are, all of us, fellow passengers on the same planet. We're, all of us, equally responsible for the happiness and well-being of the world in which we happen to live." "We have plundered it all in less than a century without paying any attention to the interests of those coming after us." Both these quotations relate to some of the statements made by George Box yesterday.

As a result of much of what has been said today, I have the feeling that many of us are acting as if we have just invented the wheel. We have only to examine the vast literature on the effects of ionizing radiation to realize how naive and inaccurate such an attitude is. Indeed in the area of radiation biometry, many concepts of interest to statisticians and environmentalists, such as doses, dose-rates and thresholds have been considered and discussed at considerable length. A National Academy of Science report released just a few weeks ago devotes an entire section to the concept of low dose. The amazing thing about this is that the committee responsible for writing the report found it necessary to devote a section to a discussion of this apparently simple concept, in spite of a fifty year history of research and literature on an agent which is known to be carcinogenic, mutagenic, and teratogenic. This report is entitled "Research Needs for Estimating the Biological Hazards of Low Doses of Ionizing Radiation". I recommend it to all serious students of the application of statistics to problems of the environment. Two other comprehensive studies of the effects of ionizing radiation are at least as important. These are:

1. BEIR; *The Effects on Populations of Exposure to Low Levels of Radiation,* National Research Council (1972).
2. UNSCEAR, A/8725: G.A. Official Records, 27th Sess. Suppl. No. 25 (1972).

MR. WANDS—During the course of our discussion yesterday and this morning, three words were bandied about—"risk," "benefit," and "analysis." So far we have focused our attention almost entirely on risk measurement and analysis but have touched very lightly, if at all, on the question of benefits. An administrator must resolve this very important side of the equation in setting some kind of regulatory standards. I grant that the data are even fewer and more unmanageable in the area of benefits than they are in the area of risk, but it is time for us to begin planning a concentrated approach to quantifying benefits. It's the old question of equating dollars with lives or marginal illness, etc., but there is still much to be done before we can achieve the long-term rational approach to the goals of which Dr. Newill has just spoken.

In response to the last speaker, one of the reasons this Symposium is being held at this particular time is because of the Environmental Mutagens Society meeting this weekend and, following that, the Society of Toxicology. This does assure a potential at least of half the interested scientific communities being in town and available, particularly since we wanted to make this Symposium nationwide rather than local as the two preceding ones were. We were very hopeful that particularly the radiation biometry group would be in our audience to share their experience with us, even though we are focusing our attention today on the problems of chemicals entering the environment.

Yesterday and again today we heard statisticians Nancy Mann and Dr. Singpurwalla mention the use of inten-

sive testing for failure as a means of predicting ultimate long life. This is fairly straightforward in terms of mechanisms that are simple and well understood, such as flex fatigue in metal strips or of paint failure under sunlight, radiation, etc. However, in biological systems one usually finds two entirely different mechanisms—one in relationship to the short-term, heavy-dose exposure and the other to the long-range, low-level exposure. Standard techniques within the field of toxicology are available for doing intensive short-term studies. Sometimes it is as short as a single dose, for example, determining an LD_{50}. More intensive, repeated doses once were used by the National Cancer Institute in its chemotherapy screening program in which animals were dosed at least twice a day for seven days at a maximum tolerated dose. We wanted the animals to stay at least barely alive so that we could study the effects of the chemotherapeutant on the animal carrying the experimental tumor. Perhaps Dr. Schneiderman would like to comment on the statistics that were used in evaluating those experiments. There is also a thirty-day feeding study which lasts a little bit longer than a single dose or a daily dosing. Sometimes this is modified by increasing the dose every week to the point of failure of the test system; ie., death of the animals. Perhaps Caroll Weil, who is in the audience and is quite familiar with the statistics commonly used in the field of toxicology might like to rise to that issue.

Last night's Washington-Star News [Mar. 6, 1974] carried in the women's section a big front color spread on the nitrite question. Attention, of course, is being focused on nitrites in our food. Two or three times during our discussions yesterday and today we have had some rather vague, but nevertheless real, suggestions that the oxides of nitrogen which are inhaled might ultimately react with some of the body proteins or amino acids to form these nitroso amines which are of concern in our diet, particularly those meat products which are preserved with nitrite. Congress has established a system for protecting the public health based upon routes by which toxicants enter our bodies. For example, FDA controls what we eat, EPA administers one law controlling the air we breathe and another controlling our drinking water. The problem is that there are many substances, such as the nitrosamines, which enter our bodies by several of these routes. Dr. Finklea gave us the example of lead in his paper this morning. Yet, there is no concerted effort to correlate the controls of these multi-entry insults to our bodies.

I would like the panel, particularly the statistician, to discuss how to tackle the nitrite problem. We know that the nitrosamines are formed in some foods containing nitrite, for which there is at present no substitute. We have been eating such foods for over a century and during that time some people have developed cancers.

DR. SINGPURWALLA—I appreciate the complexity and the magnitude of the problem, but it is not something that I can answer in a minute.

DR. HROMI—I think I can paraphrase what you said. One needs to understand what the long-range problem is before he can respond to it. That does appear to be a rather complex problem, and to try to respond on the spot is difficult.

DR. BOX—I would like to return to a question raised some time ago in the discussion by Dr. Goldsmith concerning the relation between measured levels of pollutants and levels actually breathed. One thing that is clear is that the level measured may be far less reliable than people imagine. In the records that we have been analyzing, for example, dramatic changes in apparent pollution levels can be traced to changes in location of instruments and to changes in carrying out the details of the analysis. Because reproducibility at

a given station is high, one can easily be lulled into believing that a measurement is accurate. Cooperative studies are needed on a continuing basis to provide checks.

DR. ROTKIN—I am here as an individual, so this comment is an expression of purely personal prejudice. Dr. Hromi, I was happy to hear a paper that, instead of saying what factors should be considered, actually considered them.

I don't think you should deplore adversary relations. Unless you consider them, this Symposium has an unreal air about it. Whenever you deal with problems on which people will either have to devote energy or spend money, it is unrealistic to seek the best solution from an overall humanitarian point of view. If someone must extend effort or some fortune to achieve this result, you can expect him to put up a fight to oppose it. And there is no use devising a nice procedure for helping humanity if you ignore the fact that you will get opposition—you might as well consider who will oppose you, and why, and what you can do about it. This implies adversary relations.

Concerning the indignant remark about one of the early comments regarding how long people will put up with this—will people have lost their patience? One of the first clean water acts was passed during the 19th century. People got sick and tired of their water being made dirty by all kinds of pollutants. They lost their patience again more recently when they began to find soapy foam in every stream and when several people near highways died of suffocation because of inversion. People lost their patience a long time ago—you don't have to ask when they will lose it.

It's odd that when the government or some academic group wants to conduct experiments involving humans, people worry about the ethics. Nobody considers the ethics when a manufacturer introduces a new hair spray that millions of women will breathe. Nobody worries about these guinea pigs. Nor when someone introduces a new soft drink the label on which lists water as the only natural ingredient—everything else is one chemical or another. It is made to taste like raspberry juice, but there is no shred of raspberry in it. I'm sure you can think of many other examples.

Now, my specific objection to your paper, Dr. Hromi. You objected to a ratio—you said that an arithmetic difference might be a better way to look at it. I suggest that, especially when you deal with catalysts, you should consider the possibility that deterioration will increase as the level increases. Perhaps you should have, rather than a ratio, some kind of an exponential which would make matters worse for the company rather than better.

DR. HROMI—If this question-answer period is typical, perhaps our Symposium is achieving its purpose. Being adversaries in a friendly kind of atmosphere like this is helpful. Thank you.

Occupational Exposures—Thresholds? Introduction

Bertram D. Dinman, MD

Chairman, Committee on Toxicology, National Research Council

This afternoon we address our concerns toward the area of occupational exposures; the title for this afternoon's session is "Occupational Exposures—Thresholds?" I have clearly taken a position in the past (1972) that there is such a thing as a threshold; having taken that position, I presume later I will have to back it up.

It might be useful to review previous history with regard to development of occupational standards—that is, standards for control of the occupational environment, in particular as relates to chemical agents. The first efforts were those of the National Academy of Sciences in the later '20's and early '30's, the set of volumes entitled "Critical Tables." These were rather rudimentary attempts to arrive at numbers to describe safe exposures. The next organized attempt was that of the USSR, which in the '30's developed a series of maximum permissible concentrations. This was followed in the U. S. to a limited extent by the Public Health Service during WW II which developed similar standards. But the first set of values in this country were largely, aside from the National Academy effort, due to the ACGIH, which first promulgated a series of what were called, about 1947 or 1948, Maximum Allowable Concentrations. At the same time, the American Standards Association through its Z-37 committee began the development of a number of standards. This activity persisted to some degree during the late '40's and early '50's, then became quiescent. The ACGIH (American Conference of Governmental Industrial Hygienists), a non-governmental, voluntary organization, has up to now developed a list of slightly less than 500 compounds for which a series of "Threshold Limit Values" has been developed. This term has taken the place of the term "Maximal Allowable Concentrations." The latter was replaced since there was a problem with the connotation "allowable"; ie., if you set a number and you said it was "allowable," that implied one could be "allowed" to build atmospheric concentrations right up to that level. This obviously is not the intent of any of the standards that merely set an upper limit which it would be advisable to avoid wherever feasible. The term "Maximal Allowable Concentration" still persists and is still used by several of the European countries. It has been replaced largely in the United States by the term "Threshold Limit Values." In the US for many years there were no Federal standards except those embodied in the Walsh-Healy Act and several Longshoreman Acts; such standards were essentially adaptations of the ACGIH standards. This picture of course changed with the development of the passage of the Williams Steiger Bill—the Occupational Safety and Health Act of 1970. As a result of this law NIOSH (National Institute for Occupational Safety and Health) develops documentation for the Occupational Safety and Health Administration (OSHA) of the Department of Labor, which hopefully promulgates on the basis of these recommendations of NIOSH occupational standards. As of this date they have promulgated one permanent standard for

asbestos and (famous or infamous, depending upon your point of view) the 14 standards dealing with the carcinogens. Except for these specific standards, promulgated by the Department of Labor, there are still standards based upon reference to the other consensus standards developed by ANSI, ACGIH, etc. Up until the time of the passage of the Occupational Safety and Health Act no ACGIH standards stated that the values promulgated are designed to protect "*most workers*" (and I underline and quote "most workers") during an 8-hour day, 5-day week, 40-year working life. Now the Secretary of Labor is instructed by the Occupational Safety and Health Act to set standards "such that no employee will suffer material impairment of health or functional capacity for the period of a working life."

For the sake of our discussion, and especially within this context of a concern with statistical approaches to these problems, the point of departure for my subsequent discussion will be largely the point of view taken by the ACGIH of protection of most workers, because if we are going to use as our point of departure "*no* effects upon *no* workers," then we have no basis for further discussion. We might as well close this session and go home, because I don't think there are many statistics associated with zero, although I undoubtedly will stand corrected. So, with your forebearance I will use as a point of departure the concept of "*most* workers protected," for on this basis we have room for discussion. By contrast, with *all* workers or *nobody* having any effects, there is not much room for discussion.

The scientific premise inherent in these time variables—8-hours-on, 16-hours-off—assumes that during the 16-hours-off period there will be metabolic degradative processes which should render harmless the material in question. These degradative processes obviously constitute a multivariate system. One has simultaneous enzymatic detoxification processes operating within certain rate limits, excretory activity within certain rate limits, regeneration of enzymatic detoxifications system within certain rate limits, and kinetic equilibria between circulating material and storage compartments. And, quite obviously, you have multiple variables subsumed by time and quantity considerations. Since each of these variables operate simultaneously, both independently and dependently, we are obviously dealing with a rather complex interaction. Of course there is another rather large and significant set of variables which assumes, since we are dealing with biological systems, that all of these biological functions occur in a population over some type of distribution. The implications of this will become apparent as we go on.

Those are some of the tips of the icebergs of scientific questions. Pragmatically, the fact is we don't have handles on all of these mechanisms. If one considers the problem of multiplicity, we have literally tens of thousands of chemicals to consider without data available on a quantitative basis. So, pragmatically speaking, we use what data we have, hoping that by proper experimental and statistical design we can integrate these variables in our output systems. And obviously here we have to turn to what, at the risk of being called a sexist, I will refer to as the handmaiden of the sciences—the statistical capabilities we are so fortunate to have.

Turning to another major consideration—the concept that the standard will protect most workers. Here the statistician and the toxicologist recognize an area for rather fruitful discussion. For instance, who are "most"? Each of the previous degradative systems will be a dependent function to some extent on the biologically independent variables of age, sex, and race. The second question which might be asked is, "How many are *most*?" What criteria do we apply when we say "*most* people will be protected?" Are we talking about persons who fall within one, two, or three standard deviations? Or are we referring to 90, 95, or 99% confidence limits? Obviously, these are questions that could be dis-

cussed. What I am getting to is the question of the distribution of coping with capabilities as these occur within a working population. Now, where do we stand in this regard? Previously we assumed that most people working were by implication healthy. But we know, for example, that most people over 35 years of age have latent coronary artery, atherosclerotic changes; to this extent they are not healthy. We do know those in the working population who have various other disease states, both latent and minimal. And these people will be at risk. Are these within the purview of *most*? We therefore perceive a problem here which has practical ramifications. One might consider, for instance, cost implications inherent in this process of standard setting. And each one of these considerations will have rather direct effects. In addition there are other problems to which I would allude to rather briefly. For instance, chemicals rarely exist uniquely in a working or general environment. Rather, they occur as an infinite mix of interactions—synergistic, antagonistic, additive—you name the effects. A second question that might be asked is what constitutes an effect or what constitutes toxicity? In this connection there is the position of Soviet science, which being heavily endowed with a Pavlovian point of view takes the position that an *effect* was any biological response that one could measure. By extension of the Pavlovian approach an *effect* was almost *per se* deleterious. (We have never been able to tie the Russians down to why a response *per se* is deleterious.) So the question remains—does any biological response represent the exceeding of a threshold beyond the allowable? Is a biological response an effect which must be prevented? I hope we will get some discussion to that. I have taken the position in my article in Science (1972) that "effect" is a neutral word; at least it is so stated in our dictionaries. "Effect" does not necessarily imply a deleterious resolution of a set of events.

These are briefly some of the problems inherent in this area of setting work place standards. I would hope this afternoon the panel and the members of this forum will bring these questions forward for discussion. Our first speaker is Dr. Richard Henderson, who did his undergraduate and graduate work at MIT and has his Ph.D. in Biochemistry. He was on the staff of the Biology Department there before WW II and during the war served as an Army Chemical Warfare Service officer. He returned to MIT to complete his work for his doctorate and then joined the faculty of Syracuse University where he was Assistant Professor and Associate Professor of microbiology. Dr. Henderson joined Olin Mathison in 1955, where he became manager of Environmental Hygiene services in 1962. He is presently Director of the Environmental Hygiene and Toxicology Department of Olin Matheson Corporation. In that capacity he is responsible for the planning of toxicological evaluation of new products and processes and for the control of exposure to chemicals, heat, noise, light, and ionizing radiation in Olin's operations. Dr. Henderson is a member of numerous professional societies and the author of many scientific and technical publications. I will ask Dr. Henderson to open the forum for this afternoon.

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Thresholds for Control of Potential Hazards in Occupational Environments

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The goal of an environmental hygiene-occupational medical program should be to assist individuals in the maintenance of their health. The following will concentrate on some aspects of evaluation and control of potential hazards in work environments. Before starting on the occupational factors of importance to health, one fact should be emphasized.

A review of the record of visits to the medical departments in industrial plants shows that only approximately 20% of the visits are for treatment of illness or injury directly related to potential hazards of the work environment. Headaches and upset stomachs that result from poor interpersonal relations with a fellow worker or foreman should not be considered illnesses related to potential hazards inherent in the work environment. There may be many more visits for such illnesses than from exposure to a chemical or physical agent. Such illness does need to be treated and the causes recognized and minimized both for maintenance of health and for efficient operations.

Another source of data on occupational versus non-occupational illness and injury is the record of non-occupational group sickness and disability and Workmen's (or should it be Workperson's) Compensation insurance costs. A number of industrial operations employ a sufficient number of people to obtain a rating different than the general population for non-occupational group sickness and disability insurance. The medical costs for a broken leg in a skiing accident should be about the same as a comparable broken leg from an industrial accident. There will be Workperson's Compensation payments while unable to work and there may be payment for residual disability in the case of the broken leg from an industrial accident. A review of the costs of non-occupational sickness and disability insurance and Workperson's Compensation insurance, both experience rated, has shown that the Workperson's Compensation cost is 15–20% of the total and non-occupational group is 80 to 85%. Another review has shown 876 days lost for on-job accidents and 6,022 days lost for off-job accidents (Baldwin, 1973).

We concentrate on the minor portion of the total health maintenance problem when we concentrate on occupational causes of sickness and disability. It should be much easier to measure and control potential hazards from a few chemical and physical hazards on a specific job than it is to measure and control the myriad potential hazards to which an individual is exposed off the job. Before we can control the potential hazards, we should know what they are and how to measure them.

Paracelsus wrote 450 years ago "dosis sola facet venenum," dose alone makes a poison. In terms of environmental hygiene, the *rate* of dosing alone changes a potential hazard to a hazard. Therefore, we must know what *rate* of dosing can be handled by the human body without injury. The emphasis is on *rate* because we are dealing with the dynamic system of intake-detoxication-excretion.

The Threshold Limit Values of the American Conference of Governmental Industrial Hygienists and the allowable limits of exposure established by the Occupational Safety and Health Administration for chemical substances are

expressed as concentrations, not as rates, for less than a work day. The rate of systemic dosing can be calculated from the concentration in air if the breathing rate and rates of absorption through the lungs are known. There is some information on some chemicals relative to rates of absorption through the lungs but certainly not enough to specify the range of rates of absorption over the range of temperatures, work loads, concentrations of the chemical and individual variations encountered in industry. Approximately 20 percent of the mercury vapor in a single inhalation is present in the exhaled air of a person who has not imbibed. But after a couple of beers, approximately 50% of the inhaled vapor appears in the exhaled air. How many other intakes of foods, beverages or drugs can cause similar alterations in absorption of inhaled chemicals?

We have some measure of the limits of rates of caloric utilization and hence of oxygen utilization and breathing rates. Minimum recommended daily caloric requirements of a sedentary male are approximately 2500 calories per day. The maximum caloric expenditure from continuous hard work is approximately 6000 calories per day. If 1500 of the calories in both cases are expended in the 16 hours off work, then the variation in calories expended in work may range from 1000 to 4500 for the 8-hour work day. Does the person breathing at a rate to expend 4500 calories have 4.5 times the exposure of a person breathing at a rate to expend only 1000 calories in an 8-hour work day? Or does the rate of absorption change as the breathing rate changes?

The fact that 20% of inhaled mercury vapor is present in exhaled air has been mentioned. If a person takes one breath of air containing 0.5 mg/m^3 of mercury vapor, the exhaled air should contain 0.1 mg/m^3 of mercury vapor. If this is followed by an inhalation of air containing 0.1 mg/m^3 of mercury, is any of this mercury absorbed, does it constitute a systemic dose? If a person alternately breathes 0.5 mg/m^3 and 0.1 mg/m^3 of mercury vapor in uniform breath volume at uniform rate for an 8-hour work day, is the effective exposure 2 mg hours/m^3 or is it 2.4 mg hours/m^3? Assuming we determine the effective exposure, i.e. what fraction of the exposure is absorbed, this may not be the effective systemic dose. Once absorbed, a chemical may undergo changes and the rate of change may be the limiting factor in controlling the potential hazard. The chemical that is dosed and its metabolites may have different effects on different organs.

The brain is considered the critical organ for mercury vapor and the kidney the target organ for ionic salts of mercury. Animals given a dose of elemental mercury accumulated approximately 10 times as much mercury in the brain as animals given an equal dose of an inorganic salt of mercury. This is true for both intravenous and inhalation dosing—at the rates of dosing used in the experiments (Magos, 1967; Rabinovitz, 1972; Viola and Cassano, 1968).

The rate of oxidation of elemental mercury in blood has been studied (Clarkson, et al., 1961). It is logical to assume that there is a rate of oxidation such that elemental mercury absorbed through the lungs is oxidized to ionic mercury before it gets to the brain. Elemental mercury vapor dosed at this rate would have the potential hazard of an inorganic salt of mercury for the brain.

If some of the mercury absorbed from inhalation of one breath of air containing 0.5 mg/m^3 of elemental mercury vapor travels from the lungs to the brain without oxidation but none of the mercury absorbed from inhalation of one breath of air containing 0.1 mg/m^3 of elemental mercury vapor travels from the lungs to the brain without oxidation, the two exposures can have a tenfold difference in potential hazard for damage to the critical organ, the brain. Is there a 10-fold difference in brain loading from 10 minutes exposure to 0.6 mg/m^3 of elemental mercury vapor plus 50 minutes of no exposure compared with 60 minutes exposure to 0.1 mg/m^3 of elemental mercury vapor? There are some data to

TABLE I.—Summary Tissue Analyses (from Smith, 1967).

	Hg Concentration, μg/g (Dry Weight)			
	Control	0.1 mg/m³	0.5 mg/m³	1.0 mg/m³
Kidney	23	130	428	930
Brain				
Medulla	0.1	0.2	24	55
Cerebellum	0.4	0.6	11	64
Occipital	0.2	0.4	15	84
Frontal	0.3	0.6	12	87

indicate that the brain loading of mercury is not proportional to dose but that the kidney loading is proportional in monkeys (Smith, 1971).

Kidney function tests showed no impairment of kidney function in any of the test groups. The monkeys exposed to 1.0 mg/m^3 did exhibit signs of neurological effects—shyness, irritability—in the first months of the exposure but appeared to adapt with time.

Once absorbed and distributed, mercury leaves the body via urine, feces, sweat, hair, nails and expired air. Values for urinary, fecal and biliary mercury of monkeys exposed to 0.1, 0.5 and 1.0 mg/m^3 of elemental mercury vapor are shown in Table II.

The extremely high fecal mercury values for the 1.0 mg/m^3 exposure group may be the result of ingestion of mercury during grooming; mercury condensed on fur at this dose.

Urinary mercury is used as a guide in evaluating and controlling exposure to mercury in work environments. On a group average basis, urinary mercury has

TABLE II.—Urinary, Fecal, and Biliary Mercury Concentrations (from Smith, 1967).

Group	Urine mg/l	Feces mg/kg	Bile mg/l
Control	0.03	0.36	0.12
0.1 mg/m³	0.06	0.58	0.72
0.5 mg/m³	0.17	1.56	3.73
1.0 mg/m³	1.45	54.8	14.50

been found to correlate with estimated time-weighted average workday exposure. Fig. 1 illustrates the relation. From this plot it can be determined that 0.15 mg/l of mercury in urine corresponds to an estimated time-weighted average workday exposure of 0.05 mg/m^3. On the basis of this, some persons have sug-

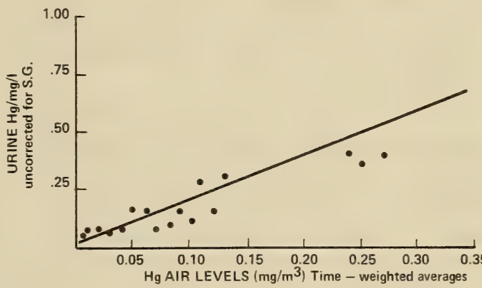

Fig. 1.—Concentrations of mercury in urine (uncorrected for specific gravity) in relation to time-weighted average exposure levels (from Smith, et al., 1970).

gested that there should be a limit of 0.15 mg/l for urinary mercury. It can be seen from Table III that a limit of 0.15 mg/l for urinary mercury would fail to detect over 60% of persons exposed to more than an estimated time-weighted average 0.05 mg/m^3 of mercury vapor for the workday. It would also erroneously detect as excessively exposed a significant fraction of persons not actually overexposed.

Another possible error in assessing potential hazard on the basis of urinary mercury is the possibility of equal urinary mercury concentration from equal doses

TABLE III.—Relationship of Mercury Exposure to Mercury Levels in Urine Uncorrected for Specific Gravity (expressed as percentage of each exposure level group within designated ranges of urine mercury levels) (from Smith *et al.*, 1970).

TWA Exposure Level Groups (mg/m^3)	Number of Workers	Percentage of Group Within Urine Level Range (mg/liter)					
		<0.01	.01–.10	.11–.30	.31–.60	.61–1.0	>1.00
Controls 0.00	142	35.2	62.7	2.1	0	0	0
<0.01	29	6.9	86.2	6.9	0	0	0
0.01–0.05	188	6.9	66.0	24.5	2.7	0	0
0.06–0.10	91	0	62.6	30.8	6.6	0	0
0.11–0.14	60	3.3	18.3	31.7	16.7	23.3	6.7
0.24–0.27	27	0	14.8	29.6	44.5	7.4	3.7

of ionic and elemental mercury that are not equal in potential hazard. There also is the possibility that the numerically equivalent exposures of 0.6 mg/m^3 for 10 minutes and 0.1 mg/m^3 for 1 hour will yield essentially the same urinary mercury values but, as indicated previously, may have different potential for damage.

The preceding has outlined some of the possible biological factors that can lead to erroneous assessment of potential hazards. Another factor that can cause error is the actual measurement of exposure. The micro-environment around a worker may have a different concentration of mercury vapor than the general work environment. This is shown in Table IV. Most published work on effects of exposure to mercury have based estimates of exposure on measurements in the general work environment. This can grossly underestimate actual exposure. Also, it is possible to have exposure continue beyond the workday from mercury on the body and clothing.

Hippocrates observed that excessive exposure to mercury appeared to be related to certain disorders of certain workers. Approximately 2400 years later we have a rough estimate of what the limit of exposure can be without damage. There is disagreement regarding the need for a greater margin of safety than that provided by 0.1 mg/m^3. Is the benefit to be derived from the increased margin of safety of a limit of 0.05 mg/m^3 worth the cost of decreasing the limit?

The effects of mercury, regardless of form, are systemic. Another example of a Threshold that does not appear to be based on mode of toxic action of the compound is the Threshold for phosgene. The effects of exposure to phosgene appear to be solely on the surface layers of the lungs without direct effect on other organs. There are effects from loss of fluid into the alveolar spaces of the lungs but the fluid and electrolyte imbalance is not sufficient to cause death. The present allowable limit for exposure to phosgene is 0.1 ppm. Several years ago the Threshold Limit Value Committee of the American Conference of Governmental Industrial Hygienists recommended that there should be a ceiling but that recommendation was withdrawn. The Occupational Safety and Health Administration limit is also 0.1 ppm. The American Conference of Governmental Industrial Hygienists recommends limiting excursions to 3× the Threshold Limit Value for periods not to exceed 15 minutes. The OSHA limit does not specify what range of excursion may be permitted for phosgene other than that the 8-hour average shall not exceed 0.1 ppm. The total exposure would be 48 ppm minutes/m^3 for 8 hours at 0.1 ppm.

TABLE IV.—Mercury Vapor Concentrations in Air Near Contaminated Clothing and Skin.

October 24–26, 1972	
Locker Room	Mg Mercury/ Cubic Meter of Air
General Room Atmosphere	0.03–0.04
Air Near	
1. Outer clothing furnished by company and laundered daily; worn one shift before measurements	0.1 –0.2
2. Gloves	0.08–0.2
3. Hands (before washing)	0.5 –0.6
4. Clean Hands (washed)	0.04–0.08
5. Sweater (employee in mercury recovery area)	0.2 –0.5
6. Rubber Coated Shoes (inside)	0.02–0.05
(outside)	0.10–0.5
7. Cotton undershirt worn approximately 6 hours in cell room. Person had no known contact of outer clothing with liquid mercury nor salts of mercury	0.01
8. Cell Room, breathing height—October	0.06–0.116
—November	0.02–0.08

A person is unlikely to breathe 48 ppm of phosgene for 1 minute, an equivalent exposure. This concentration is immediately severely irritating to the respiratory tract. A person might breathe 5 ppm for 1 minute and repeat this each hour for 8 hours; a total exposure of 40 ppm minutes/m^3. Such an exposure might cause damage. Certainly under the standard operating procedures used by phosgene manufacturers, the person who breathed 5 ppm for 1 minute would be unlikely to repeat it the next hour; he or she would be in the medical department under observation.

There are sampling and analytical methods that can detect 0.1 ppm of phosgene in a small volume of air. Air is drawn through a chemically impregnated filter paper. The colored reaction product on the filter paper can be extracted in chloroform and quantitated colorimetrically. By changing filter papers every few minutes, it would be possible to determine short-term peak exposures. The infrequent peak exposures may be more important than the uniform low level exposure relative to long-term effects of exposure to phosgene. Phosgene producers are planning a long-term study to try to improve our knowledge of the effects of exposure to phosgene.

The problem of evaluating long-term effects of low level exposure to tolylene diisocyanate are similar to those for phosgene with one exception. The allowable limit for tolylene diisocyanate is 0.02 ppm and we do not have sampling and analytical procedures to tell us whether an exposure is 0.2 ppm for 1 minute or 0.02 ppm for 10 minutes. In terms of potential hazard, these two exposures are probably different.

Congress provided enough "weasel words" in Section 6(b)(5) of the Occupational Safety and Health Act to make it possible for the Secretary of Labor to comply with the Act in setting standards. The Secretary "shall set the

standard which most adequately assures, to the extent feasible, on the basis of the best available evidence, that no employee will suffer material impairment of health or functional capacity even if such employee has regular exposure to the hazard dealt with by such standard for the period of his working life."

No matter how much data we collect and how thoroughly we apply statistical methods to the evaluation of the data, the only really meaningful datum to the individual employee is which part of the LD_{50} or effective $dose_{50}$ or other measure she or he is in. The ultimate decision of whether a potential hazard is being adequately controlled is determined by careful periodic medical evaluation of each individual. The Occupational Safety and Health Act provides for the granting of a variance when the preponderance of evidence shows that the "conditions, practices, means, methods, operations, or processes used or proposed to be used by an employer will provide employment and places of employment to his employees which are as safe and healthful as those which would prevail if he complied with the standard." If a plant has been operating unchanged for 100 years, the average length of employment has been 45 years and all retired employees have died when over 90 years of age as the result of automobile accidents that occurred on the way home from their daily 2 hours of tennis, that would probably be acceptable as a preponderance of evidence that the work environment of the plant was as healthful as it would be if an OSHA standard was met.

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Study of Long-Latent Disease In Industrial Populations

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We all recognize that our decisions concerning the control of environmental pollutants must frequently be based on incomplete information and imperfect measurement of exposure to toxic agents and of disease response. This is especially true in the case of diseases that appear many years after exposure to the toxic agent. Because lack of sufficiently detailed information may lead to misinterpretation regarding causal relationships, it is important to consider some of the pitfalls in the study of these diseases.

The material I am presenting here was prepared primarily to demonstrate how we identify occupational groups at excess risk of long-latent disease and how evidence is developed on cause-effect relationships. At the same time, because I am particularly concerned with the possible misinterpretation of epidemiological findings, I have tried to emphasize some basic principles that are frequently ignored and which may lead to erroneous conclusions of a cause and effect relationship or the lack of a cause and effect relationship.

The first point I should like to make is that in studying the relationship between occupational exposure and disease that appears many years after exposure, we are immediately limited as to the population groups to be studied, the way in which exposure can be characterized, and the disease entities to be studied. Thus, the study of currently employed industrial populations would be inappropriate for identifying effects related to exposures many years in the past, unless the turnover of the work force was extremely low. The obvious answer to this problem is to identify populations that have been employed in specific occupations many years in the past and to undertake a prospective study in retrospect of their disease experience. This we have been able to do in a number of industrial situations, including the one which I shall describe shortly. Unfortunately, such an approach imposes severe limitations on our measurement of disease response. For most industrial populations, information on the early stages of disease is available only for the currently employed survivors. Consequently, our studies of long-term disease are usually restricted to an analysis of mortality patterns, and as a further consequence must be limited to the fatal diseases in which we might expect that mortality provides a good index of disease incidence.

The population chosen for study consisted of 59,000 steelworkers employed at 7 plants of 3 major steel firms in Allegheny County, Pennsylvania in 1953. Employment records on these men were recovered in 1962 and follow-up was initiated to determine vital status through 1961. As shown in Fig. 1, only 32,263 of the original study population had continued employment into 1962. It is thus seen that a study of current employees to determine the relationship between health status and prior exposures of the study cohort would exclude almost one half of the exposed population.

Thirteen percent of the study cohort had retired during the observation

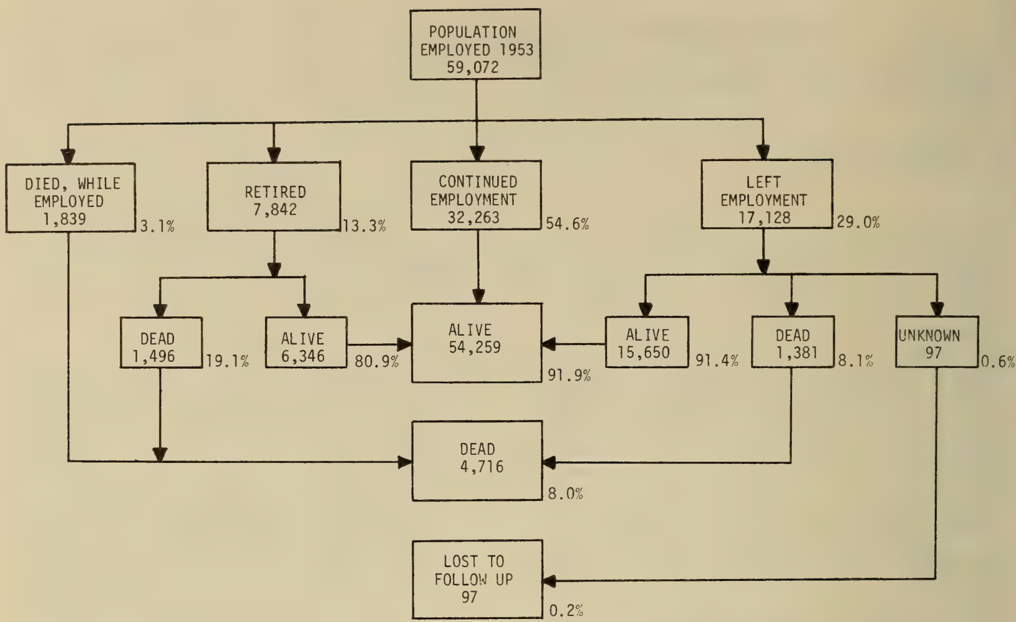

Fig. 1.—Followup of Allegheny County steelworkers population showing initial employment status and vital status at end of study.

period and the 1,500 deaths within this group could be identified by reference to company records. Another 3% of employees had died between 1953 and 1962 while employed. A frequently used approach to the study of industrial mortality is to relate these deaths to the average employment level during the period of study or to contrast the proportionate mortality due to specific causes among retirees with some standard group. Again, it should be noted that if we are concerned with relating disease response to prior exposures, we would be ignoring the experience of the 17,000 men who had left employment, and whose vital status could not be determined by reference to employer records. Extensive follow-up of those who had left employment showed that 8% of this group had expired before 1962, about the same level of mortality as seen for the total cohort. However, when we recognize that the population and deaths which could be followed through reference to employer records are heavily loaded with retirees in the older ages, it is seen that the total mortality rate for those who left employment is actually higher than for those who remained. As seen in the box at the bottom of Fig. 1, vital status was eventually determined for all but 97, or 0.2%, of the original study cohort.

A further consideration in deciding whether records available to the employer might be appropriate for the study of work related mortality, is whether these records reflect the same distribution of cause-specific mortality as would be seen in the original cohort. The figures displayed in Table 1 further emphasize the limitation of studies based on these records. Here it is seen that there is considerable variation in the percentage of deaths identifiable by employer records according to specific cause of death. As might be expected a large part of deaths from diseases of the nervous system and the circulatory system can be identified by employer records because of the high mortality from heart disease and from strokes among the retirees and men with long service. On the other hand, almost 45% of deaths due to malignant neoplasms were unknown to the employer. Even within the malignant neoplasms there is con-

Table 1.—Number of deaths from selected causes by employment status at death and knowledge of employer (Allegheny County steelworkers, 1953–1961).

Cause of Death	International List Number	Total Deaths	Known to Employer		Unknown To Employer	Percent Unknown
			At Work	Retired		
All Causes	(001-999)	4,716	1,839	1,496	1,381	29.3
Malignant neoplasms, all sites	(140-205)	1,008	212	346	450	44.6
Lung and Bronchus	(162-163)	295	68	87	140	47.5
Prostate	(177)	56	7	36	13	23.2
Brain and other nervous system	(193)	28	3	3	22	78.6
Diseases of nervous system and sense organs	(330-398)	382	136	174	72	18.8
Diseases of circulatory system	(400-468)	2,001	905	672	424	21.2
Arteriosclerotic heart disease, including coronary	(420-422)	1,680	792	543	345	20.5
Diseases of respiratory system	(470-527)	165	57	55	53	32.1
Diseases of digestive system	(530-587)	267	109	74	84	31.5
Accidents	(800-965)	356	208	39	109	30.6

siderable variation in the percentage of deaths unknown to the employer, ranging from 23% for cancer of the prostate, a group which is heavily loaded with retirees, to 78.6% of deaths from malignancies of the brain and central nervous system. It is thus seen that the mortality studies based on employer records must be interpreted with caution unless we can be assured of an extremely low turnover of personnel.

The next important factor to consider in the study of occupational disease is the choice of a control or comparison group. I think you would find general agreement in the field of occupational studies that there is no such thing as the "ideal" control population with which to compare our industrial populations. Usually it is a question of which of several comparison groups is the least biased, and in many cases the only available data for contrast is that for the general population. As a consequence, we continue to see reports in the occupational health literature of unusually low mortality for certain industrial groups contrasted with the general population, with the implication that no health hazards exist. However, when we consider that for many industrial populations a certain level of good health is required for employment and continuing good health is required for continued employment, this is exactly the pattern we would expect to see. As shown in Table 2 this is what we did observe for the steelworker population in contrast with the general population of Allegheny County. From 1953 through 1961 we observed 4,716 deaths in steelworkers where we would have expected to see 5,767. The same pattern is observed for each of the specific causes of death with the exception of accidents. This is not surprising, since we would expect more accidents in an industrial population. The extent to which such comparisons might lead to erroneous conclusions is also seen to vary both by cause and by race. For example, mortality from the infectious and parasitic diseases is less than 40% of expectation, whereas mortality from the malignant neoplasms in steelworkers is about 90% of expectation. It is also seen that the deficit in mortality is considerably greater for the non-white workers than for white, mortality for the former group being only 64% of expectation. Particularly striking is the 53% deficit for vascular lesions affecting the

Table 2.—Observed and expected deaths from selected causes by race (Allegheny County Steelworkers, 1953–1961).

Cause of Death	List Number	Total Observed	Total Expected	White Observed	White Expected	Nonwhite Observed	Nonwhite Expected
All Causes		4716	5766.8	4083	4773.5	633	993.3
Infective and parasitic diseases	001-138	65	166.9	39	94.7	26	72.2
Malignant neoplasms	140-205	1008	1091.4	861	929.0	147	162.4
Vascular lesions affecting C.N.S.	330-334	365	464.3	310	359.8	55	104.5
Heart disease	400-443	1906	2311.3	1721	2012.5	185	298.8
Arteriosclerotic and deg. heart disease	420-422	1680	2002.9	1537	1773.4	143	229.5
Other heart disease	(400-416) (430-443)	226	308.4	184	239.1	42	69.3
Diseases of respiratory system	470-527	165	237.6	136	178.4	29	59.2
Accidents	800-962	356	311.8	302	252.6	54	59.2
Homicide and suicide	(963-964) (970-985)	118	138.7	94	100.7	24	38.0
All other causes	Residual	733	1044.8	620	845.8	113	199.0

*Expected deaths calculated by applying age, race and calendar year specific rates of the study county to steelworker person years at risk.

central nervous system observed for non-white steelworkers.

For study of specific occupational groups within the steel industry, we have used the total steelworker population as the control group. This is, of course, preferable to comparison with the general population in that it overcomes selection related to employability. At the same time, it should be recognized that other selective factors may be operating. Principal among these is selection for health which may be expected to occur within specific work areas. Thus for certain occupations that are very demanding and require good health for initial and continued employment, the less healthy individual would be expected to choose work in other areas or, by company policy, be assigned to less demanding areas when ill health develops. Consequently, certain work areas may show marked deficits or excesses in mortality unrelated to exposure in the occupational environment.

Another important and frequently overlooked factor to be considered is that striking differences in mortality may be expected by chance alone when we make many comparisons. This is seen in Table 3 which displays some of the comparisons for men employed in 1953 in each of 53 work areas within the steel industry. Although such a division is not unexpected, it should be noted that almost half of the work areas show a lower mortality rate than expected (28 above and 25 below). Because of concern with potential hazards in the occupational environment, many studies of occupationally related disease have been directed at the identification of employed groups showing excess disease, and deficits have been mostly ignored. However, deficits in mortality may indicate the selection of more healthy individuals into certain work areas and, as a possible consequence, the assignment of less healthy individuals to other areas. For that reason, an evaluation of the relationship between the area of employment and consequent mortality must include an assessment of both excesses and deficits. We note in Table 3 that significant excesses in total mortality were seen for janitors and men working in the machine shop, while a significant deficit in mortality is noted for men working in the carpenter shop. The excess for janitors was predictable, and is not related to occupational exposures. Rather, the high mortality for this group is due to assignment of workers from other areas because of ill health. We should also note in Table 3 that the

Table 3.—Number employed in 1953, observed and expected deaths from all causes and standardized mortality ratios by work area (Allegheny County steelworkers, 1953–1961).

Work Area	Number Employed	Observed Deaths	Expected Deaths	SMR[1]
All steelworkers	58,828	4,685	4,685.0	100
Annealing normalizing	1,367	121	109.2	111
Batch pickl. - sheet dry.	76	6	5.8	103
Billet bloom & slab	3,136	290	277.5	105
Blacksmith shop	271	26	28.2	92
Blast furnace	3,455	244	265.6	92
Carpenter shop	406	29	40.9	71*
Coating	332	19	23.4	81
Coke plant	2,552	206	198.9	104
Cont. pickl. & elec. clean.	205	12	9.8	122
Cold reducing mills	1,499	100	87.1	115
Electric main. assg.	1,616	118	127.3	93
Electric furnace	742	46	43.8	105
Electric shop	561	39	49.5	79
Foundry	1,143	90	101.1	89
Gen. admin. & clerical	3,312	224	240.8	93
Gen. finish & ship.	473	38	40.2	95
Gen. receiv. & stores	196	17	19.0	89
Gen. technical	2,373	123	130.1	95
General labor	969	84	78.4	107
Heat treat. & forging	1,015	75	78.1	96
Hot pack mills	572	55	66.6	83
Hot strip mill	161	19	17.0	112
Hot strip rolling	1,014	63	73.9	85
Janitors	521	113	84.5	134**
Loco & car repair	129	11	11.7	94
Machine shop	1,450	151	130.2	116*

[1] $SMR = \frac{\text{Observed deaths}}{\text{Expected deaths}} \times 100$

Significance based on summary chi-square with 1 degree of freedom.

* 5% level

** 1% level

mortality for coke plant workers, which will be discussed in detail, appears to be little different from that of other steelworkers.

One of the most serious deficiencies in the study of long-term disease is that we only rarely have access to measurement data which depict the extent of exposure to specific toxic agents in the work place. In the present study, for example, degree of exposure could only be characterized in terms of length of employment within specified work areas and in a few cases by reference to recent measurements within smaller sub-divisions in these work areas. As crude as these measures are, we would expect that they would reflect any dose-response relationship unless there was a rapid turnover of the work force due to early removal of "more sensitive" individuals. An example is shown

in Table 4 where we have noted the work areas which show a significant excess or deficit in total mortality for persons employed five or more years in each work area. Here it is seen that significant differences in mortality have been noted for several of the work areas which were not noted in Table 3. An interesting observation, for which figures are not shown in Table 3, is the 25% excess in total mortality for white steelworkers employed as general laborers. White steelworkers employed in this area in 1953 showed a slight deficit in mortality during the study period. This suggests that the inclusion of many short-term employees may have masked important differences. We also notice in Table 4 that the total mortality experience of white and non-white workers employed at the coke plant for 5 years is quite different. While the non-whites show a 22% excess in total mortality, the experience for the white workers is about as expected.

Another problem in limiting our analysis to persons employed in a single year, such as 1953, is that persons suffering ill health may migrate to other work areas. Consequently, we may draw erroneous conclusions about health status from our observation of the more healthy survivors. To illustrate how extensive this problem might be, we show in Table 5 the distribution of workers employed in the 2 major subdivisions of the coke plant in 1953 and in prior years. This subdivision into coke oven and non-oven areas was based on previous information which suggested that excess mortality of coke plant workers might be associated with exposure to coke oven effluent. Of the 59,000 steelworkers employed in 1953, 2,552 were working in the coke plant. However, an additional 978 men employed in other areas in 1953 had been employed at the coke plant in some prior year. Thus, limiting the study to those employed in 1953 would exclude 28% of mean with prior coke plant exposure. It is also seen that the distribution of white and non-white workers is considerably different for oven and non-oven areas, and that the proportion of coke oven workers

Table 4.—Persons years at risk, observed and expected deaths from all causes and standardized mortality ratios for men employed at least 5 years in specified work areas (Allegheny County steelworkers, 1953–1961).

Work Area	Race	Person Years at Risk	Observed Deaths	Expected Deaths	SMR
Batch pickling-sheet drying	Total	525	18	9.9	182*
Carpenter	Total	2,963	30	44.4	68*
Coke Plant	White	11,549	130	130.8	99
	Nonwhite	6,509	96	78.7	122*
Cold reducing mills	White	8,204	71	70.7	100
	Nonwhite	593	14	6.3	222**
General finish. and ship.	White	4,385	69	64.4	107
	Nonwhite	1,041	8	16.9	47*
General labor	White	4,989	102	81.6	125*
	Nonwhite	3,636	49	46.1	106
Janitors	White	1,117	54	34.5	157**
	Nonwhite	2,015	33	33.1	100
Maintenance NOS	Total	2,246	10	28.9	35**
Mech. main. assg.	White	22,313	377	318.4	118**
	Nonwhite	721	16	14.3	112
Merchant mills	White	18,372	253	266.9	95
	Nonwhite	2,966	28	42.4	66*
Sheet fin. and ship.	White	13,466	158	133.5	118*
	Nonwhite	438	5	7.3	68

* 5% level
** 1% level

Table 5.—Distribution of Allegheny County Coke plant workers by work area, race, and period of employment.

	Coke Plant	Coke Oven Number	Coke Oven Percent	Nonoven Number	Nonoven Percent
		Employed in 1953			
Total	2,552	1,327	52.0	1,225	48.0
White	1,645	520	31.6	1,125	68.4
Nonwhite	907	807	89.0	100	11.0
		Employed in 1953 or Prior Years			
Total	3,530	2,048	58.0	1,482	42.0
White	2,369	993	41.9	1,376	58.1
Nonwhite	1,161	1,055	90.9	106	9.1

excluded by an analysis limited to 1953 is greater for whites than for non-whites. Overall, 35% of men with prior coke oven exposure would not be included in such an analysis, while 42% of white coke oven workers would be excluded.

Mortality from specific causes for the coke oven workers is shown in Table 6. Here it is seen that of the 184 deaths among men previously employed at the coke ovens, only 100 deaths would have been related to this area in 1953. As regards the cause-specific mortality of these workers, it is seen that the greatest part of the excess mortality for the non-white coke oven workers is due to a significant excess in respiratory cancers. While the differences are not significant, the respiratory cancer mortality for white coke oven workers also suggests the possibility of an excess risk for this disease. Looking now at Table 7, we see that the only suggestion of an

Table 6.—Observed and expected deaths and standardized mortality ratios for men employed in 1953 and in prior years (Allegheny County coke oven workers, 1953–1961).

Cause of Death	Employed in 1953 Observed Deaths	Employed in 1953 Expected Deaths	Employed in 1953 SMR	Employed in 1953 or Prior Observed Deaths	Employed in 1953 or Prior Expected Deaths	Employed in 1953 or Prior SMR
			White			
All Causes	25	31.4	80	80	80.2	100
Malignant neoplasms-respiratory system	3	1.9	-	8	5.0	160
Malignant neoplasms-digestive organs and peritoneum	1	2.4	-	6	6.1	98
Other malignant neoplasms	2	2.1	-	6	5.8	103
Vascular lesions affecting CNS	1	2.3	-	6	6.4	94
Heart disease	9	12.8	70	29	32.9	86
Diseases of respiratory system	0	1.0	-	2	2.5	-
All other causes	9	8.9	101	23	20.5	112
			Nonwhite			
All Causes	75	65.2	115	104	92.9	112
Malignant neoplasms-respiratory system	17	5.7	298**	25	8.4	298**
Malignant neoplasms-digestive organs and peritoneum	3	4.1	-	3	5.7	53
Other malignant neoplasms	8	5.6	143	10	8.3	120
Vascular lesions affecting CNS	6	5.6	107	9	7.8	115
Heart disease	13	18.9	69	20	27.6	72
Diseases of respiratory system	4	3.0	-	5	4.2	119
All other causes	24	22.4	107	32	31.0	103

Table 7.—Observed and expected deaths and standardized mortality ratios for men employed in 1953 and in prior years (Allegheny County non-oven workers, 1953–1961).

Cause of Death	Employed in 1953			Employed in 1953 or Prior		
	Observed Deaths	Expected Deaths	SMR	Observed Deaths	Expected Deaths	SMR
			White			
All Causes	89	89.7	99	119	113.6	105
Malignant neoplasms-respiratory system	1	5.7	18	3	7.3	41
Malignant neoplasms-digestive organs and peritoneum	10	7.4	135	14	8.8	159
Other malignant neoplasms	5	6.0	83	4	8.2	49
Vascular lesions affecting CNS	12	7.2	167	11	8.9	124
Heart disease	38	37.4	102	58	47.6	122
Diseases of respiratory system	6	3.1	194	5	3.9	128
All other causes	17	23.0	74	24	28.9	83
			Nonwhite			
All Causes	17	12.6	135	17	13.8	123
Malignant neoplasms-respiratory system	4	1.1	-	1	1.2	-
Malignant neoplasms-digestive organs and peritoneum	2	0.8	-	3	0.9	-
Other malignant neoplasms	3	1.2	-	2	1.4	-
Vascular lesions affecting CNS	0	1.1	-	2	1.2	-
Heart disease	4	3.9	-	4	4.1	-
Diseases of respiratory system	2	0.6	-	2	0.6	-
All other causes	2	3.8	-	3	4.4	-

excess for respiratory cancer was in the non-whites who worked in the non-oven area in 1953. Since men employed only in the non-oven area in 1953 or prior years show a deficit for respiratory cancer, this would suggest that these deaths are associated with exposure at the coke ovens in years prior to 1953. Analysis of the data with reference to place of employment in 1953, therefore, would have attributed these deaths to exposure in the non-oven area.

Further subdivision of the coke oven population according to exposure levels, as defined by work assignments and years of employment, demonstrates the relationship between exposure to coke oven effluent and lung cancer response. This is shown in Table 8. Here we see that the excess of lung cancer is associated with 5 or more years employment at the coke ovens and that the level of risk increases with amount of exposure, the greatest exposure to effluent being at the top side of the ovens. These findings also serve to illustrate how occupationally-related disease might be masked by limiting study to broad occupational groups. Since the top-side workers constitute only 15% of coke oven workers and only 9% of coke plant workers, the extremely high risk for top-side workers is reflected as a considerably lower relative risk for coke oven workers and coke-plant workers. If the lung cancer rate for top-side workers had been only double the rate for other steel workers, we would have failed to note a significant excess for coke oven workers, while a 5-fold risk would have been insufficient to demonstrate a significant excess for coke-plant workers. A similar diluting effect may result from inclusion in the study group of coke oven workers with too few years of observation to allow for the appearance of latent effects.

Finally, it should be pointed out that before we draw inferences about causation, we should, to the extent that information is available, rule out other factors which might explain the relationship. While we could not identify any artifactual relationship which would produce such a unique picture only for Allegheny County, it was suggested that replication of this study in other geo-

Table 8.—Number employed, observed and expected deaths and standardized mortality ratios for selected causes by length of employment and work area in 1953 and prior years (Allegheny County coke oven workers, 1953–1961).

Work Area and Length of Employment	Number Employed	All Causes Observed Deaths	All Causes Expected Deaths	All Causes SMR	Malignant Neoplasm of Lung Observed Deaths	Malignant Neoplasm of Lung Expected Deaths	Malignant Neoplasm of Lung SMR
Coke Oven, < 5 Years	1,144	67	72.8	92	4	4.7	-
Coke Oven, ≥ 5 Years	904	117	100.3	117	27	7.6	355**
Side Oven Only	496	53	55.1	96	6	4.1	146
Side and Topside	276	29	27.9	104	6	2.1	286**
Topside	132	35	17.4	201**	15	1.5	1,000**
		Other Malignant Neoplasms Observed Deaths	**Other Malignant Neoplasms Expected Deaths**	**Other Malignant Neoplasms SMR**	**Diseases of Resp. System Observed Deaths**	**Diseases of Resp. System Expected Deaths**	**Diseases of Resp. System SMR**
Coke Oven, < 5 Years		7	10.5	67	0	2.8	-
Coke Oven, ≥ 5 Years		20	16.4	122	7	4.0	175
Side Oven Only		7	8.9	79	3	2.3	-
Side and Topside		9	4.3	209*	3	1.0	-
Topside		4	3.2	-	1	0.7	-

graphical areas would provide additional evidence on the subject. Further study of coke oven workers throughout the United States and Canada was undertaken and some of the findings are shown in Table 9. Here it is seen that coke-oven workers from other geographical areas also experienced unusually high lung cancer mortality; that the highest risk is observed for the top-side workers; and that men employed at the coke ovens also are at excess risk of genito-urinary cancer. A second point to be considered in determining causality is whether the

Table 9.—Observed and expected deaths, 1951–1966, and relative risk for selected causes for coke oven workers employed during 1951–1955 at 10 non-Allegheny County plants for coke oven workers employed during 1953 at 2 Allegheny County steel plants.

Cause of Death	Full Topside Obs. Deaths	Full Topside Exp. Deaths	Full Topside Rel. Risk	Partial Topside Obs. Deaths	Partial Topside Exp. Deaths	Partial Topside Rel. Risk	Side Oven Obs. Deaths	Side Oven Exp. Deaths	Side Oven Rel. Risk
All Causes	157	144.1	1.12	73	69.2	1.07	359	379.7	0.92
Malignant neoplasms-lung, bronchus, and trachea	35	10.4	7.24**	7	3.7	2.14	27	19.4	1.73*
Malignant neoplasms-genito-urinary organs	4	2.5	-	2	0.8	-	15	9.2	2.02*
Other malignant neoplasms	15	16.8	0.86	5	9.7	0.44	35	41.4	0.79
Tuberculosis of the respiratory system	3	2.3	-	0	0.3	-	5	4.8	1.06
Other diseases of the respiratory system	7	7.0	0.99	5	3.1	1.78	18	17.8	1.01
Cardiovascular renal diseases	60	68.5	0.84	39	33.8	1.18	155	180.9	0.80*
Accidents	10	13.4	0.72	7	7.4	0.94	45	40.1	1.19
All other causes	23	23.1	1.00	8	10.2	0.76	59	66.2	0.85

Significance of Relative Risk (Rel. Risk) based on summary chi-square with one degree of freedom.

* 5% level

** 1% level

\- less than five deaths

findings are consistent with other comparable observations. A review of the literature shows that all of the occupational groups engaged in the carbonization of bituminous coal or in handling of the by-products are at excess risk of cancer for 1 or more sites. More specifically, all of the 3 populations engaged in the carbonization of bituminous coal have shown a striking excess of lung cancer and the lung cancer response is positively associated with the temperature of carbonization.

One final factor which must be considered, because of the recognized association with lung cancer mortality, is the possible role of cigarette smoking in the unusual mortality experience of the coke oven workers. Unfortunately, as with most long-term studies, no smoking histories are available. However, it is possible to determine by reference to lung cancer mortality rates for cigarette smokers in the United States whether the unusual lung cancer experience of the coke oven workers might be explained by differences in smoking habits. As seen in Table 10, while the total steelworker population shows a lung cancer mortality somewhat like that observed for all cigarette smokers, and the coke-oven workers, never topside, show rates not too different from those for heavy cigarette smokers, the rates for top-side workers and for those employed more than 5 years top-side are far beyond what would have been predicted by differential cigarette smoking experience. While the possibility of a synergistic relationship between cigarette smoking and exposure to coke oven effluent certainly cannot be ruled out, we can say that the unusual lung cancer mortality experience of the coke oven workers cannot be accounted for by cigarette smoking habits alone.

Table 10.—Estimates of average annual lung cancer mortality rates (per 100,000 person-years) for selected U.S. smoking groups, 1954–1962 and steelworker groups, 1953–1961.

AGE	U.S. Smokers 35-44	U.S. Smokers 45-54	U.S. Smokers 55-64	U.S. Smokers 65-74	Steelworkers <45	Steelworkers 45-54	Steelworkers ≥55
Never smoked or occasional only	-	-	12	29			
Current cigarette smokers - total	5	39	158	258			
Current cigarette smokers, 1-9/day	-	-	69	119			
Current cigarette smokers, over 39/day	-	104	321	559			
Steelworkers					12	126	160
Coke oven, never topside					-	130	387
Coke oven, topside					228	1,058	1,307
Coke oven, ≥5 years topside					265	1,587	1,961

Occupational Exposures—Thresholds?
Panel Discussion

Chairman: **Dr. Bertram D. Dinman,** *Aluminum Company of America*

Panelists: **Dr. Samuel W. Greenhouse,** *National Institute for Child Health and Human Development*
Dr. Richard Henderson, *Olin Corporation*
Dr. J. William Lloyd, *National Institute for Occupational Safety and Health*
Dr. Charles Powell, *National Institute for Occupational Safety and Health*

DR. GREENHOUSE—The chairman and I have agreed that I will not speak for more than 15 minutes. This agreement was arrived at on the assumption that 15 minutes was a good threshold level above which we will observe detectable behavioral physiological effects on the audience and below which we will not observe any adverse effects at all. This is the definition that Dr. Vaughn Newill gave yesterday morning. I hope you deduce from what I said that I don't believe in threshold levels.

Dr. Henderson's paper with regard to his subject matter is one on information—obtaining knowledge on setting standards. Thresholds are involved. These are matters in which it is very difficult to arrive at reasonable decisions. These are problems related to environment, and the statistical roles in these problems are much larger than that. I agree with Dr. Newill in that "threshold" is not a very useful concept scientifically in the sense that fever, for example, isn't a very useful concept. Dr. Henderson made some statements which I think may gain him enemies. Answers to questionnaires do not necessarily reflect the real changes included in his actual statement. The entire Census Bureau would be up in arms against the implications of that statement. Are synthetics in the environment worse than the natural ones? Well, I'm not so sure. This may be a judgment on Dr. Henderson's part. I interpret it as a judgment. I'm not sure we have any evidence that this is indeed so.

DR. HENDERSON—I do not object to the use of questionnaires and answers to questionnaires as one means of obtaining data. I do object to opinions expressed in answer to questionnaires being accepted as fact. The number of "yes" answers to the question on a medical history, "Have you lost weight?" was found to correlate with estimates of exposure to elemental mercury vapor during working hours. This was a one-time medical history. Actual weight measurements over ten years did not show any weight changes that correlated with exposure to elemental mercury vapor.

Persons who have cited the paper reporting the findings on the "yes" answers to the questionnaire have indicated that exposure to elemental mercury vapor causes loss of weight; this is not what the authors of the paper stated, although this is what the figure summarizing their data indicates.

DR. GREENHOUSE—Dr. Lloyd's report is remarkable as an epidemiological study as it does not reflect a planned experiment. There is no randomization, according to Prof. Box, and as a result one must be very careful in analyzing the data. Conclusions and inferences are very tricky. Dr. Lloyd has taken all

the necessary precautions in arriving at conclusions, and he is left now with the really meaningful effects. He is to be congratulated because these are the kinds of studies that we are going to have to depend upon more and more. This is a wide open field. To me, the basic issue is the relationship in a mathematical sense between two areas: (1) our agents and (2) the human subject. There are lots of problems with regard to the agent: How much is there? Does it act by itself? Does it interact with other agents? There are lots of problems in regard to the host—the human being. One of the medical, physiological and biological parameters that we have to be concerned about is whether we know enough about biological systems and how they react with drugs and agents and pollutants. We still don't have answers to the vital problem, namely, what are the relationships between the two and how does one influence the other in terms of changing the health status of our population or affecting biological systems which may be detectable only in future generations. This is the fundamental issue.

It reminds me about the story of a lad going through life asking one question to which he cannot get an adequate answer. He asked his grade-school teachers, and when he got into high school he asked his high school teachers, and when he went to college he asked his professors. He asked his mother and father. Everyone said, "I don't know—I can't give you an answer." He said, "This is the sole question I have to ask of God—how can I look up a word in the dictionary if I don't know how to spell it?" Essentially the ingredients to that question are the ingredients to our problems. Statisticians have a tradition for not being associated with research inquiries which are labeled "fishing expeditions." How much of what we've heard yesterday morning and what I've heard at other symposia on the environment (this is my third) has focused on measurement of the agents? I hope many of you were fortunate to hear Prof. Box's talk at the Berkeley Symposium in which he gave details about the Los Angeles data. They are remarkable data. The analysis was remarkable. He had graphs with black dots starting at 6:00 a.m. representing hourly observations of the increase in atmospheric pollution as traffic started moving, and from the ocean up to the hills of San Bernardino depending upon whether the sun was shining, the nature of the winds, and so on. Remarkable. But I'm sure that Prof. Box will be the first to agree with me that that's only the beginning of our problem—namely, to discover these techniques of analyzing from time series or other approaches—analyzing problems related to the agents and tying them in to the problems related to the human being. Now we are falling short of this in most instances except for epidemiologic studies of the kind that Dr. Lloyd talked about.

We have very serious problems from a statistical point of view. I do not think personally that we can have randomization. I can't for the life of me see how we're going to randomize human individuals so that one stays within one mile downwind of a nuclear power plant and another stays two miles downwind or east or north or south, and then wait 20 years to discover the effect of these various dosage levels. Essentially that is what we are getting at—an attempt to get a dose response curve through natural observations. This leads me to another point. There are a lot of loose references to terms which ought to be defined very carefully. Many people have different conceptions of what an experiment is. When you live 25 years with a group of intermural scientists—I call them whitecoats—you learn that an experiment is a very deliberate, planned intervention of a human being into a natural phenomenon. The use of the word "experiment" this morning was ambiguous. It wasn't clear whether people meant that kind of intervention by investigators, or they were

talking about observations of the natural course of events. I think it would be best to call these observational studies. Some of these have been well thought out, well designed, and well planned in advance. Others, of course, have been very sloppy. There are biases in all of these things. The best planned prospective study with human societies involving randomization has very serious problems from the point of view of the basic issue—that is, long-term follow-up of individuals who are subjected to low doses of contaminants in our environment.

I definitely believe our problems with food supply may be even more serious than with pollutants in the air. It seems to me that this issue is a significant one from the point of view of statisticians. People come to a symposium with different expectations. Some of you may have come here hoping that statisticians will give you a way to get at these relationships—the bridge between the agents and the human individual. I'm not so sure that we have these methods on hand. In the absence of randomization, prospective studies have biases just as serious as retrospective studies. We are dealing with phenomena which in many instances have incidences of 6 per 1,000 or 6 per 10,000—for example, the incidence of stroke in young women between the ages of 16 and 19. I shouldn't say incidences, because mortality from stroke in young women between the ages of 15 and 19 is something like one in a million. It is inconceivable that any Federal agency, at this point in time, can devote a prospective randomized study to determine the effects of oral contraceptives in increasing excess risks for stroke. Yet we have seen some additional cases of stroke in young women in which retrospective studies combined with some estimates of relative risks have been disregarded by many mathematical statisticians, who go to the extremes of saying the information cannot be used as cause and effect—*ergo*, no decisions on the basis on the part of regulatory agencies should be made. One of the biggest shibboleths, I believe, after many, many years of confrontation with this issue, is "cause and effect." I think we tend to throw the term around very carelessly and in many instances unknowingly as to what cause and effect really is in biological phenomena. We think we know what it is in physics. I'm not sure we know what it is in the biological sciences. We know there are some people with a disease who do not show the symptoms. The counter argument is that they have not lived long enough. I don't understand what that means and yet we talk about cause and effect as if this is to be the great counter to the statistical studies that come up with so-called associations. Now, statistical associations may be the only way we can arrive at answers to relationships. It is in this very field where I predict the future of statistics is going to be very bright, because no scientists have been able to obtain answers to these questions—the long-term effects of nitrogen oxides or sulphur dioxides on man. Laboratory scientists may be able to tell you the effect of varying dosage on a cell or tissue, but they will not be able to tell you what it is on the living human organism, and only statistical studies will be able to give an answer. This will revolutionize the status of statistics in science. To the man in the laboratory for many, many years statistics was a way of doing t tests in the analysis of variance. Statistics will come into its own right by providing the fundamental knowledge required to solve this particular problem.

There are serious problems with regard to the agents, and symposia should start zeroing in on what those problems are and what methods are required for solving them. There are serious problems with regard to the human individual—namely, what biological systems do we *a priori* on theoretical grounds think will be affected by these various chemicals, then try to zero in on identifying them.

Then there will be statistical studies which may be long-term. I don't see how they can be short-term.

DR. DINMAN—Thank you, Dr. Greenhouse. I couldn't agree with you more. As a practicing toxicologist of sorts I become utterly desperate when faced with laboratory data on animal models upon which we are supposed to take action to protect human health. This is obviously the most ethical approach to solutions of these problems, which are real for myself and for society.

DR. POWELL—Thank you, Bert. After the discussion we just had and two very fine papers, there is really very little left that can be said. In that regard I take the position that at best it is a matter of pursuing scientists and at worst, anti-scientist. Let's start looking at some very basic questions. When we start to collect data and analyze it, what is our purpose? Dr. Henderson was obviously developing some information that could be utilized for standards. Dr. Lloyd, in his paper, had done a tremendous job in identifying a very big problem area. Now we are suddenly in a position where professional judgment may play a lesser role than before because we are faced with standards. If I had any criticism of Dr. Henderson's paper, it would be that the very role we are playing today is the one in which we are faced with standards, yet we still must apply our own professional judgment. You can't really call that a criticism, but it is a quandary we are faced with—OSHA versus preventive medicine, if you will. When you look at the OSHA standards and what they have had to start with, they picked those things that were available to them. The present standards don't give us the flexibility that we have always exercised. I think we are in a true transitional period. When you look at this kind of transition and how the data is presently being utilized, you recognize suddenly that when you wrote that paper five years ago that really wasn't what you intended at all, but somebody has picked it up and is now utilizing it to develop a standard. Being involved in criteria development puts you in one mell of a hess because the data isn't the kind that you would have liked to have to do that kind of job with—it's skimpy at best. Dr. Lloyd had a very large group to work with, he was able to form many different subgroups, and he was able to identify the problems for us, but he wasn't able to identify any kind of dose response or cause-and-effect relationship. We're not even sure what it is in coke oven emissions that's causing the problem. I shouldn't say yet because there are probably a number of things that are acting and reacting with one another. Oxides of nitrogen is another case in which large group studies will be necessary. Yet when we look at oxides of nitrogen individually, we can't even tell whether they are acting alike—in fact, we suspect that they aren't. So large groups are needed to solve these problems because it is only statistically that you can look at them effectively.

Let us go back to Dr. Henderson's paper. He had a rather small group and was trying to develop a cause and effect. We have talked about the data that he has collected a number of times. Most of us recognize that he has begun to make a very significant contribution. For years we have been trying to relate environmental factors to biological monitoring and it just has not worked out. You can do it in a group but not individually. I don't think you would want to use that kind of data for standards, which then goes back to the transition we are looking at. For Dr. Henderson or others to use the kind of information he has developed on an individual plant basis is extremely good, but to try to apply it as a national standard sends chills up both our backs, I think. I would like again to reinforce the concept that we are in a transition—we are moving into something entirely different—and we may have to start looking at the kind of data we collect a little differently than in the past. It's not a matter of individual scientists looking at papers and

using their own professional judgment—suddenly it will become something we have to live with whether we like it or not. These two papers emphasize this point better than anything that I could continue to say.

DR. DINMAN—This session is titled "Occupational Exposures—Thresholds?" I detect among us a common concern about whether there is such a thing as a threshold. For instance, Dr. Henderson points out that the weight change recorded was a response to a questionnaire and that perhaps from a quantitative point of view there are more objective measurements of weight change. Well, objectively you are quite right. We can measure weight loss or weight gain, and indeed that is an effect. But one of the premises in standard health protection is that we will have the information upon which to act long before any severe debilitating effect occurs. Therefore, one might ask, if one were to wait for weight change of an objective quantitative nature, whether or not we would be meeting that objective. On the other hand, weight gain on a questionnaire basis can be approached from a totally different point of view. What is perceived as a vague dys-ease—I'm not saying "disease"—may express itself in weight gain or weight loss. It may express itself in a malaise which certainly can't be quantitative, yet we know in preclinical phases before significant damage is done to the individual, such findings almost inevitably occur. Of course they occur due to other causes, too. But that's not the point. We are looking for these early changes. In a sense Dr. Lloyd is also talking about the same thing. The threshold and response to excess mortality suffers from similar problems as to the detection of a threshold for morbidity, as Dr. Henderson pointed out. How much of a signal amplitude is necessary before we arrive at detection? I agree with Dr. Greenhouse that in this convergence of statistics and human biology and epidemiology, be it retrospective or prospective, that we do have the mechanism—regrettably it is a *post hoc* mechanism after the damage has been done—for definitively developing association. However, the point can be made that the signal-to-noise ratio here—except in such very unusual events as a bladder tumor or an angina sarcoma (perhaps two or three cases in the U.S. a year)—the epidemiologic tool was also faced with this problem of the sensitivity of the threshold for detection of the signal over the background of considerable noise. Unless you have this unusual situation of an almost unique event, then the threshold for detection and response by the epidemiologic method does suffer this imperfection. Now I suppose one can go from this to the laboratory setting.

DR. GREENHOUSE—Concerning the small incidence versus the rare event versus the more common event, I think you will find statisticians are not stupid people. When you are dealing with phenomena which occur with relatively large frequency—one in a 100—in which, in 100,000 people you may expect 1,000 cases per year with a two-fold increase in risk, so that you want to distinguish between the 1,000 or 2,000, everyone would agree to do the study and do it in the best way possible. But here we are talking about cancers, tumors, and other phenomena which are rare—one in 1,000 or one in 10,000. This issue becomes a very fundamental one. Let me repeat again what I may not have made very clear. When we are talking about low-dose phenomena we are concerned about rare-phenomena events. If they were not rare we wouldn't have this trouble because the increased risks would already have been detected. You wouldn't have to go to your statistician to assess the impact of an increase in the pollutants in the air if they affect phenomena which occur frequently. That I think is the fundamental point.

DR. GOLDSMITH—Dr. Dinman may have inadvertently pointed out a little difficulty in talking about threshold problems when he talked about signal and

noise and detectability of rare events. It seems to me that there are two or three very commonly used terms which the toxicologist has every right to use and to encourage others to use—"threshold" and "synergism". The toxicologist who designs an experiment with different dosages appropriately related to one another is perfectly entitled to say he has identified a threshold when he finds no effect of the type he is looking for at a level of such and such, but at a somewhat higher level he does find an effect. It is logically and scientifically appropriate to say that between those numbers there is a threshold. If a toxicologist finds that two agents together produce an effect which is greater than either of them would produce alone and that their effect together is greater than the sum of those effects, he is properly talking about synergism. However, when we talk about the human population exposures, we are not dealing with planned experiments and we should not use either term, in my opinion. This is true especially when we have not very clearly specified the agent as unfortunately is often the case with community air pollution or with such occupational exposures. In the study of human population exposures we are not dealing with an experimental system. The number of variables to be considered becomes potentially infinite, although Dr. Newill gave a list of 10 classes of variables. The variables are not under the control of the scientist. He must accept the intrinsic variation, must accept perhaps the fact that in some occupations there are more cigarette smokers than in others. He must decide how he will handle this factor; either by getting smoking histories or talking only about non-smokers, and so forth. Now, if there is a single agent under the control of the technical process manager, the threshold notion may be useful. I am not trying to deny its utility. But when one begins to extend this notion to combined exposures where they are not subject to the control of any managerial force, then in my judgment we are not using these terms with precision.

With many of the pollutants for which we have been collecting, over the years, a great deal of information—radiation, carbon monoxide, lead, and mercury, for example—we still have doubts about the applicability of the threshold concept. The mere fact that we have been collecting information about these four for a long time doesn't assure us that we really understand a great deal about them with respect to general population exposure. My personal experience has been with only two of these—lead and carbon monoxide—and I think from the research that I and my colleagues have done in the last decades we have evolved different views about where the important biological effects are occurring. In the case of lead in the last decade we've paid more attention to hemo-synthesis and less to wrist drop and colic. In the case of carbon monoxide we've paid more attention to cardiovascular disease and less to losing consciousness. But for whatever it's worth, just because we have been collecting a lot of information for a long period of time about an agent, we must not pretend that we really know enough to say at what level we ought to intervene or at what level we do not need to intervene because the risk of ignorance is small. The concept of the thresholds for these exposures may no longer be appropriate. An argument can be made for the difficulty in establishing a threshold for lead and carbon monoxide. We know how to draw a line which will clearly enough distinguish which people might be harmed and which people might not. I think that the toxicologist is perfectly entitled to talk about thresholds and to get other people to apply what he has learned. When we start talking about general community exposures I think we must be willing to ask a more sophisticated set of questions.

DR. DINMAN—I really can't disagree with you. Indeed, there may be individuals in the total population whose cardiovascular status is so compromised that the addition of one molecule of carbon monoxide is sufficient to plunge him

over the brink. And I think the point is well taken that we are not operating in a uni-agent environment. Now, on the other hand, there is something more than just synergism or indeed additive effects. There is the other side of the coin, of course. There are antagonistic effects and there are subtractive effects. As to matter not having enough knowledge, I think we are between the Scylla and Charibdes of two positions, one which represents the position of Kierkegaard when he speaks of the paralysis of knowledge—that we don't know enough, the other which says "don't do something—just stand there." So we are between these two extremes. I don't know the answer to whether we have enough knowledge about CO or lead. I think, however, society is asking us to take some position, some action. I think we are faced with this responsibility to respond.

DR. SCHNEIDERMAN—I want to rise to your bait of how many are "most" when we talk about most workers not being harmed by some level of a pollutant. Let's use the current issue of the excess of angiosarcomas in the PVC workers as an example. I don't remember the number that have been discovered so far among the PVC workers—six, seven, eight?

DR. DINMAN—Eight.

DR. LLOYD—Ten in the United States at this point.

DR. SCHNEIDERMAN—Ten in the United States at this time. I would then say that ten of 4,000 to 15,000 workers who have been exposed is being interpreted as exceeding the level allowed for "most" workers not to be damaged. It seems to me that the number "most" (or really 100% minus whatever percent "most" represents) is some very small number and it gets very close to the zero workers harmed that you can't talk about. Angiosarcoma of the liver is a very rare disease. There may be more than two a year here in the United States (aside from these exposed workers) but probably not more than 20. It would seem to me that we really are getting very close to zero when we are concerned about 10 in 15,000 and thus very close to what the law requires—ie., no one harmed. The nonofficial group whose standards OSHA has accepted has talked about "most" workers being unharmed in spite of the fact that the law talks about all workers being unharmed. So in terms of public reaction to this kind of exposure and illness, I would say that "most" is only infinitesimally different from "all".

Dr. Lloyd, you broke down your workers into those exposed for more than 5 years and those exposed less than 5 years. Do you mean to imply that 5 years was a safe level and that a guy could work topside for 5 years at no risk? Because if that's so, then you've got a threshold.

DR. LLOYD—I would say no. The primary reason for picking a number like 5 years is because we would expect a smaller effect at that level. Inclusion of large numbers of workers with short exposures or insufficient latency might dilute to the extent of masking significant differences.

DR. HENDERSON—We have to look at this question of "most" and "all" in terms of the requirements of industry. At least one of the factors we must consider is hiring and lack of discrimination. One of the exercises we have just been going through is looking at thresholds in terms of women in childbearing age, where the risk to the women of childbearing age may be greater than the risk to older women, women with hysterectomies, or males. I wonder, then, whether we can expose them alike or must we discriminate against them in that situation? We've gone through a similar exercise in terms of the glucose-six-phosphate-dehydrogenase deficiency that shows up in 10–12% of American negroes and under 1% of American whites. On theoretical grounds at least they are probably more susceptible to exposures to the methemoglobin-forming compounds. We have had a genetic

screening program but we have had to argue on the basis of fair employment practices in order to do that. We were actually challenged by the NAACP that we were discriminating when we started to do this and we had a job of explaining, and I understand the case was dropped. But you see as long as we can exercise selection of our work population, we're probably not too badly off. However, when you have the post coronary patient which you are reluctant to take back because of a possible aggravation of his pre-existing condition, and you don't have another place to assign him, then that actually gets in the way of utilizing the limited productive capacity of somebody who already has some disabilities. We are faced with hiring the whole person and anything that happens, if it is aggravation, we are responsible. We get into cost-benefit ratios in terms of a lot of people. How many people are we going to force out of work if we make the standards for all so stringent that it costs us too much to enforce it? If we have to take these people who have a higher-than-usual risk when we may be able to accept a standard on a selective population, we may have costs that exceed benefits.

UNIDENT.—One response that one has to make to Dr. Schneiderman is in reference to the question of protection. First of all, this statement comes from the ACGIH list—you will find in the ACGIH publication chemicals but not carcinogens, except for one.

DR. SCHNEIDERMAN—Hasn't ACGIH set a standard for asbestos exposure?

UNIDENT.—Well, there is also a standard for asbestos according to OSHA—like get down to one particle.

DR. SCHNEIDERMAN—Asbestos is another carcinogen, in addition to nickel carbonyl.

UNIDENT.—OK, but all chemicals in large enough doses will produce toxicity. All chemicals in large enough doses won't produce carcinomas. The Hartwell and Shubik list is a long one but not as long as the lead toxic substances volume which is put out annually by NIOSH. When you are talking about most carcinogens you are quite right —this is unacceptable. But there are other things in this world than carcinogens.

DR. DOMEY—You just said that large enough doses of any chemical entity would cause some sort of disorder, but that in some doses they would not. Will you name one?

UNIDENT.—Oxygen.

DR. DOMEY—Another one.

UNIDENT.—Water.

UNIDENT.—Salt.

DR. DOMEY—Another?

UNIDENT.—Sugar.

DR. DOMEY—Another?

UNIDENT.—Alcohol.

DR. DOMEY—Some of the ones you have been naming have been cited as possible toxicants. But you have not named even one of the toxicants mentioned before—hydrocarbons, nitrous oxide, and the like.

DR. HENDERSON—There is an FDA proposal for labeling oxygen—do not use for longer than one hour at a time.

DR. DOMEY—Of course the . . . .

DR. HENDERSON—It is identified as a toxic agent.

DR. DOMEY—A moment ago you said that at high levels almost any agent will produce deleterious effects. At some reduced level some will not produce carcinogenic effects—no, deleterious effects. Please name one.

UNIDENT.—Vitamin A.

DR. DOMEY—Would you be prepared to defend that?

UNIDENT.—Yes.

DR. DOMEY—Very well, then, is that a standard?

UNIDENT.—Well, if they are present at a level at which one can detect the deleterious effect, then there must be a standard.

UNIDENT.—It's a threshold.

DR. DOMEY—The threshold would become the standard.

UNIDENT.—Well, no. No, no, no.

DR. DOMEY—Well, wouldn't you say that at the point where the agent was determined to cause an effect, it would be deleterious? If it was deleterious, then would you say that we ought to erect defenses against it?

UNIDENT.—With vitamins there are some standards that are set by the FDA—no more than X amount a day.

DR. DOMEY—Yes, well, then why do we attack, for example, the concept of standards? One sees here that you are having some difficulty in accepting the idea of a threshold.

UNIDENT.—We don't.

DR. DOMEY—Then if there is a threshold, why can't there be a standard set around it?

UNIDENT.—That's what we are doing.

DR. DOMEY—But apparently there is a general sentiment abroad that these threshold are not settable.

UNIDENT.—We don't acknowledge . . . . .

DR. DOMEY—Then it is a matter of obtaining data.

UNIDENT.—True.

DR. DOMEY—Then why don't we obtain it? If so, then we are back to the circularity of the thresholds-standards problem.

DR. GREENHOUSE—That's the heart of the problem. If you are at that low a dose, the effect you are going to expect will be very small and it may take a long time to detect it.

DR. DOMEY—Well, then, why don't we do it?

DR. GREENHOUSE—That is a rational argument. You might have changed your question as follows: "For no carcinogenic agents are there threshold levels?" Then there is another mode of reasoning. The definition used yesterday is a difficult one in a sense that if you have a minimum dose above which you begin to see clinical signs of cancer, then it follows almost immediately that if, before that dose there was an onset of a sub-clinical latent period in cancer, that level as a threshold is meaningless. Now let me say again what I said once before. The concept of a threshold is not an unimportant concept. To argue about it is, I think, an important thing because it is very difficult for agencies to obtain—they have to have standards, they have to set arbitrary thresholds. There must be a decision made somewhere for regulation purposes. Industry will argue with the agencies as to whether the standard is appropriate or not. The scientific issue of the threshold does us no good. We might just as well explore other issues.

DR. DOMEY—Except there is the question of . . . .

DR. GREENHOUSE—The dose response curves, for example.

UNIDENT.—Yes, but there is the question of demonstrating statistically, which is about all one can do—that an event is not the cause. Didn't you say that if under x, y, z circumstances you cannot demonstrate that, an effect "x" is present?

DR. DOMEY—Only within a probability limit.

UNIDENT.—Well, certainly within a probability limit.

DR. DOMEY—There will be only one person in a hundred billion that will react.

UNIDENT.—So we are debating whether there will be standards set with which we can negotiate risk.

DR. DOMEY—If you can identify large toxic doses, then why don't you test backward until the deleterious effects disappear and infer your threshold and standard that way?

UNIDENT.—Well, why not?

DR. GREENHOUSE—You are zeroing in on the heart of the problem, Bert. You see, if a phenomenon occurs with very rare frequency, p is equal to 1:100,000.

UNIDENT.—Surely.

DR. GREENHOUSE—And if you are going to explore 5000 individuals in an industry your chances of finding it are zero.

DR. DINMAN—And it is conceivable, then, that perhaps we should shift to methods for establishing a way of negotiating with the general public. If within some degree of reason we can explain to the workers the varieties of risks which they may take in some particular job, then we would involve them in a decision-making process which might please them. If, for example, if we could say in some graded response that x, y, z condition is more risky than another, perhaps we could compensate them differentially for the varieties of risk. That is merely widening the range in which we speak.

DR. GOLDSMITH—The Occupational Safety and Health Acts specifies one other thing that you have just hit upon—each employee shall be informed of the toxic effects of excessive exposure to a material for which there is a standard, shall be informed what the standard is, and also shall be promptly informed when he is being overexposed and what is being done about the overexposure. The Act makes it a responsibility of the employer to inform the employee of what the risks are. This is something that we include as part of the training program in any operation. Now when you do this with a high risk material, especially in case of an accidental spill or a break in a pipe, some people will opt not to work in that particular job. So that is part of the hiring practices and you face that. But the employee by act of Congress must be informed in this decision-making process and has the choice of whether he wants to work in that particular environment.

DR. GREENHOUSE—In response to that last comment, there is a lot of confusion about what I think was manifested in that discussion. The objectives of this Symposium are not to talk about thresholds, nor to talk about Federal agencies being correct or incorrect, nor whether industry is doing something correct or incorrect. The objective of this Symposium is to bring people together to contribute knowledge accumulated since the last Symposium. About Dr. Henderson, for example, who initiated a study which says: Given such and such a concentration of mercury in the inhaled air of my workers, what is the effect on their health? That is the objective of this Symposium—that and nothing else. All the other things are peripheral, and when you say they're peripheral it doesn't in any way lower their significance. Obviously, in the political arena the setting of a standard is extremely difficult and from the point of view of industry economically extremely important. But from the point of view of scientific gain of knowledge the objective is clear. How do we establish the impacts of concentrations once we know how to measure them on the health of our people? I must say, one of the deficiencies in my discussion is that I should have taken two moments to indicate the ideal way in the best of all possible worlds of getting at this question. Remember, the idea is to discuss the *ideal* method and procedure for obtaining an assessment of impact—the effect of these pollutants on increased disease incidence.

One of the big areas that hasn't been mentioned here enough (Dr. Schneider-

man may have mentioned it yesterday afternoon) is the teratogenic effect. For six years I have been trying to set up a registry of congenital malformations—presumably, any possible effect that undue concentrations of various contaminants and pollutants in the atmosphere would have on pregnancy and on the fetus. Very difficult to do—can't even get it off the ground. That hasn't been mentioned as a serious impact. Reports from CDC in Atlanta mentioned 7 monsters being born in one week in North Dakota (or South Dakota). Everybody observed airplanes with DDT. When epidemiologists investigated this, they found that there was no excess risk in terms of excess probability due to chance. But finding these 7 in one week once causes one to take account of the appropriate defined area, appropriate population, etc. These are the difficult problems that I think are the objectives of this Symposium—tying in, knowing how, what are the best techniques, are there any techniques, what can be done to obtain a procedure which will say that after a 10-year exposure to such and such a concentration of this pollutant we found no ill effects, we found an increase or double increase in cardiovascular disease, 1.5 increase on the risk of cancer of the bladder, etc., etc. Those are the things we have to get at if we are going to know what we are talking about. If we had these dose response curves, administrative decisions would be quite simple and quite easy. Despite the fact that I say it is simple to formulate what we need, I'm not so sure that the implementation may ever be performed.

UNIDENT.—In view of Marvin's talk yesterday and Dr. Rall's and Dr. Lloyd's talks today on cancer mortality, I think the picture is misleading because unless you can demonstrate that the degree of mortality is directly related to the incidence, you may not be getting the correct and proper information at all. Among other things, many cancers are treatable—many people who have cancers don't die of cancer. You're not taking these cases into account. There is also the very proper question, I believe, of the effect of concurrent disease on mortality due to cancer or related disease. I think these things have to be taken into very serious consideration.

DR. LLOYD—One of the very first statements I made was to this point. When we are looking at long latent disease we would like nothing better than to be able to see these people when they are alive and try to intervene. The records just don't exist anywhere—that is one of the problems we have to attack. On the question of how cancer mortality reflects the incidence of the disease, it depends on what we are looking at. If we are looking at angiosarcomas of the liver—count the dead ones—they don't live very long. That's still pretty well true for lung cancer incidence. It is no longer true for skin cancer anymore—we couldn't do a study like that on skin cancer.

UNIDENT.—A great variety of cancers depend upon what the survival rates look like. Until we set up some way of getting this information during the morbid state before we perceive death, I don't know any way to attack the problem. I know we would have never identified the problem if we had been trying to count the people who were coming down with lung cancers, because most of them had long departed from the distilling industry.

DR. DINMAN—I would like to thank members of the panel and the speakers this afternoon. I must say that the charge Dr. Greenhouse suggests we should take sounds like the charge of a working committee of seven maids with seven mops and seven years worth of time. That does not minimize its value, however. This afternoon I have been reassured, because up until a few months ago I was not too sure that there were such things as thresholds. Everybody seems to agree it is not worth even arguing about. I have found it to be a rather important issue in fact. I must say we made some progress.

Summary Session Introduction

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Good morning ladies and gentlemen. On behalf of the Program Committee I'd like to extend a special welcome to you this morning. Our numbers are somewhat reduced at the end of a three-day symposium. The diehards really are with us. This is appropriate because we view our discussions today as constituting essentially a summary and a wrap-up session. Therefore I shall take advantage of this opportunity to talk to you from the standpoint that the Program Committee adopted a certain philosophy for this Symposium which may be somewhat in conflict with the views that have been expressed by certain participants up to this point. Let us speak to the problem of the nature of this Symposium for a bit—the philosophy behind it and potentially how we will move forward into the future.

As a preliminary remark, I might also note that yesterday's panel ended on the question of "what is the purpose or the theme of this Symposium?" Moving directly to the set of biases that the Program Committee expressed on this point, I'd like to review with you the fact that each session has had program elements on stage which were carefully selected for given purposes. You will note that in each of our major scientific sessions there was a biological scientist, skilled in research and focusing on problems of the environment; a statistician whose emphasis on problems of the environment had been demonstrated; and a political figure—a person engaged in duties of legislative or regulative character who required useful interaction with two prior disciplines that preceded him on the program. Sometimes the order of presentation got a little inverted. From the Program Committee level, we felt that these three disciplinary areas of national importance with respect to chemicals, man, and the environment should be brought together in the context of cross-fertilizing the approaches to the solution of national problems.

Now obviously this interaction must begin with a man who is blessed or cursed with duties in the legislative or regulatory area. He desperately needs information which will permit him to make judgments on very important topics. These topics embrace the effects of chemicals on man in terms of safety, risk, hazard, permissible levels of exposure. In order to make effective and sensible judgments with respect to these very important topics he needs, above all, data which pertain to low-dose versus response relationships in a test organism as close to man as possible. The levels or doses tested should be so low that the effects observed are almost at the minimum level. Not quite zero, as Dr. Dinman said yesterday, but quite close to zero. In effect the legislator or regulator needs information about the shape of the dose response curve for a particular response to a particular chemical agent at doses which are so low that the effects are marginal. In this Symposium you have heard that what we are worried about is detecting tiny response signals in the presence of background noise which may be large enough because of biological variability to obscure the signal. Dr. Dinman made this point clearly. So we view the legislative regulator as

requiring this kind of information as his chief tool in looking at the problem of safety, chemicals, man and the environment. I believe it was Dr. Worcester who made this point first and most strongly on the first day. Thereafter the idea, which is quite correct and important, became submerged in a sea of adulation about the beauty of collecting data on the human by the epidemiological route. We would like to resurrect the idea and bring it forward. One of the prime points of the morning is to look at the important problem of dose versus response curves in biological systems including man, at very low concentrations, because it is quite clear that the shape of that dose response curve at the point at which the response takes off from zero is terribly important in defining or making sensible use of the word threshold, which has been mentioned often in this Symposium. So at this time we must view the generation of important dose vs. response data at the very lowest end of the curve as one of the scientific elements of highest priority in dealing with the problem of contamination in the environment.

At this precise point, then, one has to consider the entrance of the biologist and the statistician into the planning of experiments which lead to meaningful probing of the reactions of chemicals in animal models, to get to meaningful data which will cast light on effects at very lowest level of discernment. The Program Committee viewed one of its prime tasks in setting up this Symposium as that of bringing the biologist and the statistician together in advance, if possible, for joint analysis of available experiments which have led to meaningful data bearing on the problems of health, welfare, man, and chemicals in the environment. We weren't entirely successful, I fear, in bringing about the interaction of the biologist and the statistician *a priori,* but we view it as a matter of interest in the ultimate success of the interaction if this meeting triggers the biologists and statisticians to get together for effective planning in the future *before* the experiments begin, rather than after. The prime reason for this is that the low-dose sector of the response curve is the most expensive, the most tedious, the most laborious, and the most difficult part to investigate. Consequently, excellent planning is required to make appropriate use of resources. And that's my lead-in to introduce to you this morning a man who is one of those entrusted with highest national responsibility for meaningful work leading to evolution of information on the low-dose versus response part of the curve. This information is entrusted in its execution to a center called the National Center for Toxicological Research, in Jefferson, Arkansas. This morning we are extremely pleased to welcome its distinguished director, Dr. Morris Cranmer, to talk to us about the basic problem of low-dose versus response and include in his discussion, I hope, the wonderful concept of the megamouse experiment. Dr. Cranmer.

Reflections in Toxicology

Morris F. Cranmer, PhD.

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Thank you for your kind introduction, Doctor Friess. I will try to weave a few of my own comments and opinions with comments by previous speakers and observers into a review of what has been stated or implied. I will also try to document what research the NCTR is doing that may impact on these problem areas. I have borrowed a few slides from previous speakers to emphasize certain points.

Few people will dispute the fact that chemical technology has in large measure contributed to the achievement of our present standard of living. Accompanying these benefits, however, are the many subtle and sometimes gross effects that are threatening the health of our society. The existing implications on future generations demand the adoption of a rational policy on chemical utilization that will enable the highest possible standard of living accompanied by an acceptable risk-to-benefit ratio.

Persons suffering from an incurable, fatal disease would not want to have treatment with a drug withheld because of a potential danger of developing cancer far into the future. Similarly, persons beyond the reproductive years certainly have less concern for exposures to chemicals that produce birth defects or genetic change than do young adults. In short, society accepts considerable risks when the risks are necessary and when acceptable alternatives do not exist, but is predictably unwilling to accept risks when the information quantitating those risks is not available.

Efficacious products are generally approved for use if there is an acceptable safety margin between anticipated residues, by appropriate usage patterns, and that level estimated to be safe to humans. The toxicologist is faced with the dilemma of estimating risk to an enormous and variable human population from small numbers of highly controlled experimental animals. Thus, there exists a considerable potential for error in assessing the risk/benefit ratio under present conditions.

Several facts which contribute to the uncertainty of toxicological evaluations should be stated clearly. There is no way to guarantee absolute safety! Small populations of experimental subjects, either animal or man, provide an imprecise basis for comparison to a large human population of variable genetic/disease states, cultural backgrounds and ages. Toxicological assessments are made singularly and humans are exposed to a milieu. It should be equally clearly understood that a proper experimental design will minimize noise and maximize comparisons and that we are constantly expanding our toxicological armamentarium. Doctor Rall explored many of the strengths and weaknesses of toxicological evaluation in his paper on Wednesday morning.

Is the toxicologist faced with a paradox of absolutes? What are the approaches available in attempts to generate reasonable policy and guides for chemical use and control? An examination of the involvement of the toxicologist in guaranteeing an adequate and acceptable food supply will be illuminating. There are three major control strategies available for the regulation of toxicants, including carcinogens: 1. the all-or-none approach, (for example, the Delaney Clause of the Food, Drug, and Cosmetic Act [FD&C Act]); 2. the use of safety factors (commonly applied to non-

carcinogenic lesions); and 3. statistical extensions beyond the experimentally observable range. Each approach has its proponents and critics, its advantages and disadvantages. Doctor Schneiderman made several comments on the Mantel-Bryan model for extrapolation, and I will try to expand this point.

All-or-None Approach

The Delaney Clause of the FD&C Act is an all-or-none approach and an understanding of complications in the quest of absolute safety is required. The Delaney Anticancer Clause contains two main segments; one for human and one for animal food additives. The segment addressing human food additives states that in evaluating the safety of such compounds used in food-producing animals, consideration must be given to the safety from possible residues in the products of those animals which are a source of food for man. When there is insufficient evidence to establish that a finite or negligible residue of the compound is safe in human food, or when the anticancer clauses contained in sections 409(c)(3)(A), 512(2)(1)(H), and 706(b)(5)(3) of the Act are applicable, a zero tolerance (no residue) must be required. Under the provisions of the anticancer clauses, no compound may be administered to animals which are raised for food production if such compound has been shown to induce cancer when ingested by man or animal, unless such compound will not adversely affect the animal and no residues, as determined by methods of analysis prescribed or approved by the Secretary (DHEW), are found in the edible products of such animals under conditions of use specified in labeling and reasonably certain to be followed in practice.

How Does One Establish the Toxicity; e.g., Carcinogenesis of a Compound?

A protocol advanced by the National Cancer Institute for carcinogen screening calls for 50 male and 50 female animals to be tested at or near the maximum tolerated dose and a like number at half that dose. A maximum tolerated dose ideally would be that which does not kill the animal except via tumor production in significantly less than a normal lifespan. The choice of using high doses is a statistical expedient in order that high incidences of tumors above background can be detected with small sample sizes. There is no biological basis for use of high doses. Such high doses may completely alter metabolic pathways, absorption and distribution.

Positive and negative results are treated in a completely different manner. If the screening test shows positive carcinogenic action, under the Delaney Clause there is no alternative but to ban the compound even if more than adequate information was available to perform a risk/benefit analysis. All too often, a negative result is interpreted as indicating a noncarcinogenic compound. The negative cannot be proved statistically. Thus, it is common practice to take an arbitrary fraction, say 1/100, of the minimum "no-effect" dosage as safe. Again, the minimum "no-effect" dosage is ill-defined and is a random variable depending on the number of animals tested. Two statements will be repeated in this and subsequent discussions. First, it is argued that such an approach has worked over the years. Second, do we have epidemiological evidence to show that no small increases in cancer have resulted from such environmental chemicals? Until recently we were not aware of the vinyl chloride problem even though billions of pounds are manufactured each year. Most new chemicals have not been in the environment long enough for effects to be noted where long latent periods may exist.

Zero Tolerance—No Residue

If one defines zero as complete absence, the dilemma of a no-residue concept quickly takes form. First, let us consider the ways by which one might attain the complete absence of a residue. The first would be to never allow contact, and the second would be to consider a rate of

removal or transformation that after a given waiting period would result in complete removal.

Since the first approach effectively eliminates the use of a chemical, let us proceed with the concept of removal. The process of removal may be passive, e.g., the removal of a persistent pesticide from a food by rain or washing, or it may be via active excretion or metabolism. If an enzymatic process is involved, the process will be accelerated.

The rate of an enzymatic process can be either zero ($^{dc}/dt = Ko = KoC^{\circ}$), first order ($-^{dc}/dt = k_1OC = k_1C^1$), or second order ($-^{dc}/dt + k_2 (C \times C) = k_2C^2$). If one solves these equations for time needed to achieve the removal of the last molecule, it is a very long time indeed. Further practical complications arise via the determination of analytical methodologies which would be acceptable for determining zero. To carry the discussion to the extreme, we would have to analyze the complete extract of the complete sample with an analytical sensitivity of one molecule.

We will probably all agree that there are certain advantages to the use of biologically active chemicals and there is also the need to insure that the population is not exposed to hazardous residue levels. One reasonable approach would be to insure the absence of any hazardous quantity of a residue. The advantage of this approach would first be to fix the amount of residue which is expected to be hazardous and at the same time determine the sensitivity of a method required for analysis to support regulatory actions. Rephrased, requirement for methodology would be defined by need rather than state of the art. Use of a chemical would not be approved until acceptable methodology was developed.

As is often the case, problems are not solved, they simply are reshaped, for we now have the problem of determining the acceptable residue level.

The Delaney Clause, or any similar all-or-none approach, is likely to be inadequate in two respects. First, it provides a false sense of security by ignoring the problem of "false negatives" which may result from inadequate testing. The FDA is charged with the responsibility of attempting to minimize such occurrences, but the question remains as to how to best accomplish this formidable task. Second, because of current toxicological ignorance we have little to offer as a substitute for the Delaney Clause which requires banning of food additives shown to be carcinogenic in animal tests. However, with adequate data, yet to be produced, the benefits of a food additive in preventing food poisoning, for example, might be documented to far outweigh a carcinogenic risk which may occasionally occur only late in life.

Safety Factors

Safety evaluation at the present time is founded on the concept of the "Maximum no-effect dose." The procedures are designed to determine the intake over extended periods (including a lifetime) that will not produce the injurious effects characteristic of the substance when given in large, that is, toxic amounts. Also important is the exclusion of the possibility that these "subtoxic" amounts will produce some hitherto unsuspected reaction. A summary of the kinds of specific studies usually undertaken can be found in the paper by Friedman and Spiher *(FDA Papers,* Nov. 1971).

The unique difficulties in safety evaluation arise from the unusual goal of attempting to prove scientifically that no deleterious effect has taken place, i.e., to prove the negative. Experiments are usually designed to establish that phenomena, apparently resulting from experimental manipulations, are real, are not artifacts or have not occurred simply by chance. On the other hand, the more appropriate concern would be to ensure that the *absence* of positive findings (*assuming adequate protocols and procedures*), is not due to chance or to the *inadequacies of sample size*. Pursuing this point supports the awareness that positive findings may be artifacts and

therefore adequate probing of techniques and replication of experiments to verify findings is mandatory. Insistence on any desired degree of assurance against making a wrong conclusion is standard operating procedure. Conventionally, a statistically significant finding must have a probability of no more than one chance in twenty of being a chance occurrence, and often risks of only 1 in 100, or 1 in 1000, or less, are desired. Clearly the severity of an all or none approach to avoid the risk of a *false* positive reinforces the desire of a petitioner for the clearance of a compound. Have we dealt equally with false negatives?

A practical approach for dealing with these uncertainties for noncarcinogens has been the use of the 100-fold margin of safety. Substances to be added to food should not demonstrate an effect in animals when fed at a dose at least 100 times greater than the likely human exposure. Our intuition tells us that this approach has usually worked very well; however, we should not forget the absence of an experimental or theoretical basis. When followed blindly, rather irrational experimental practices, interpretation and rationalization can be made.

There have been attempts to apply safety factors to carcinogens in our food supply. One of the latest discussions was by Carrol S. Weil (1972 *Toxicology and Applied Pharmacology, 21*(4): 454) where a safety factor of 5,000 was suggested. Weil argued, as had Friedman, that it was contrary to "scientific judgment" to try to extrapolate mathematically beyond the range of experimental observation. Weil suggested that it was, however, more scientific to use a safety factor of 5,000.

The application of a safety factor established from a "no effect level" in a toxicological evaluation has a number of pitfalls which were succinctly summarized by Weisburger and Weisburger (1968, *Food Cosmetic Toxicology*, 6: 235–242):

> It seems to us a "no effect dose" for a carcinogen is a highly relative level which applies only for the precise experimental conditions generated. While similar considerations hold for drugs, the risk is not nearly so intense. More often than not, an improper dose rate for rapidly acting drugs is detected almost immediately and appropriate remedial action can be taken. With chemical carcinogens and their long, latent period, the disease condition resulting from inappropriate selection of dose levels and alteration of environmental conditions leading to potentiation may become visible only years after the exposure. At that time remedial action is obsolete and often worthless.

It is necessary to add to the Weisburger remarks that a no-effect dose, with the exception of a threshold, is sample-size dependent and therefore is *not* some absolute reference point.

A number of the contributors and observers, including Doctor Worcester and Doctor Henderson, during the discussion periods on Wednesday and Thursday, spoke of thresholds.

A few comments on "threshold" are an appropriate prelude to a discussion of methods for mathematical extrapolation. The concept of a threshold dose is based on the premise that a smaller dose will not produce an effect. There are several problems with demonstrating the reality of a threshold. More refined methods of observation may lower the observed threshold; repeated examination of the bioassay will demonstrate variability even within the same individual, and heterogeneity of the population will influence the responses observed. Many toxicologists have stated that for any compound there must be a "biologically insignificant dose." There is little doubt that this is true; however, what is our definition of insignificant. A case in point are reports which have been used to estimate that 3–5 percent of those people hospitalized have drug complications severe enought to extend their duration of care, a very ominous statistic.

Mathematical Extrapolation Models

One of the real pleasures of my scientific career has been being associated with discussing mathematical models with Dr. Dave Gaylor, and much I will say is his work. Due to, at least, the toxicological uncertainties of extrapolating risks from relatively high experimental dosages in animals to low human exposure levels,

there are many people who propose complete prohibition when a chemical is demonstrated to be a carcinogen. A modification would be to use a conservative method of linear extrapolation from an upper confidence limit on the experimental result back to a zero response at zero dosage. This procedure is described by Gross, Fitzhugh, and Mantel (1970) and the FDA Advisory Committee on Protocols for Safety Evaluation (1971). This procedure is based on the premise that at low dosages, many dose-response curves are concave upward and a straight line is a conservative upper limit to such curves. In the simplest case with a single dosage and no spontaneous background occurrence of tumors, the extrapolation would proceed from setting an upper confidence limit on the observed tumor rate at the experimental dosage, d, and constructing a line back to zero. Such a straight line is likely to be above the true dose-response curves at low doses. For low dosages, the one-hit curve is approximately proportional to dosage (linear).

For the particular experimental conditions, a conservative upper limit, p_0, can be estimated for any low dosage, d_0. If a threshold dosage does exist below which no tumors are produced, the true tumor rate at d_0 may be zero. An objection to this method of linear extrapolation is that in order to obtain small risk levels, p_0, the levels of d_0 which could be tolerated often would be too small to make the food additive effective for its intended purpose. However, this procedure encourages better experimentation in that as the number of animals tested is increased, the upper confidence limit will generally decrease thereby increasing d_0 for any given level of estimated risk, p_0. The more complicated and common situations of non-zero spontaneous background and multiple dosages are discussed by Gross, Fitzhugh, and Mantel (1970).

Of the common mathematical models often proposed for extrapolation (one-hit, logistic, extreme value, and probit) the Mantel-Bryan (1961) procedure proposes the use of the probit.

The model for extrapolation usually cannot be determined from experimental results. For example, consider the probit, logistic, and one-hit curves which all give a 50% tumor response at a unit dose and 16% tumor response at 1/4 that dose. These curves would be indistinguishable in the 8% to 92% tumor response range as usually observed in experiments. Several thousand animals would be required to distinguish between the probit and logistic curves in the 2% to 4% response range with no guarantee that either model would be applicable at lower levels. In Table 1 are shown extrapolated doses producing small risks where the experimental data appear almost identical in the 8% to 92% tumor response range (FDA Advisory Committee on Protocols for Safety Evaluation (1971)).

Table 1.—Doses required to give low estimated risks from experimentally indistinguishable results with 8–92% tumors (a dose of one unit produces 50% tumors).

Estimated Risk	Probit	Logistic	One-hit
10^{-3}	1.5×10^{-2}	3.1×10^{-3}	1.4×10^{-3}
10^{-6}	1.4×10^{-3}	9.8×10^{-6}	1.4×10^{-6}
10^{-8}	4.1×10^{-4}	1.6×10^{-7}	1.4×10^{-8}

For example, if a dose of one unit produced 50% tumors, then a dose of .015 units would be expected to produce 1 tumor in 1000 animals, assuming extrapolating with the probit curve. Extreme differences between models in estimated doses are noted when extrapolating to a 1 in a million risk. The "extreme value" curve, another possible model, would generally lie between the probit and logistic, depending on slopes, Chand and Hoel (1973). Thus, the choice of a model for extrapolation is extremely critical, the one-hit being the most conservative and the probit the least conservative of those examined here.

Fig. 1 illustrates procedures utilized in the Mantel-Bryan model. The Mantel-Bryan (1961) procedure utilizes a linear relationship between probits and log dosage. They propose a conservative slope of one probit per 10 fold reduction

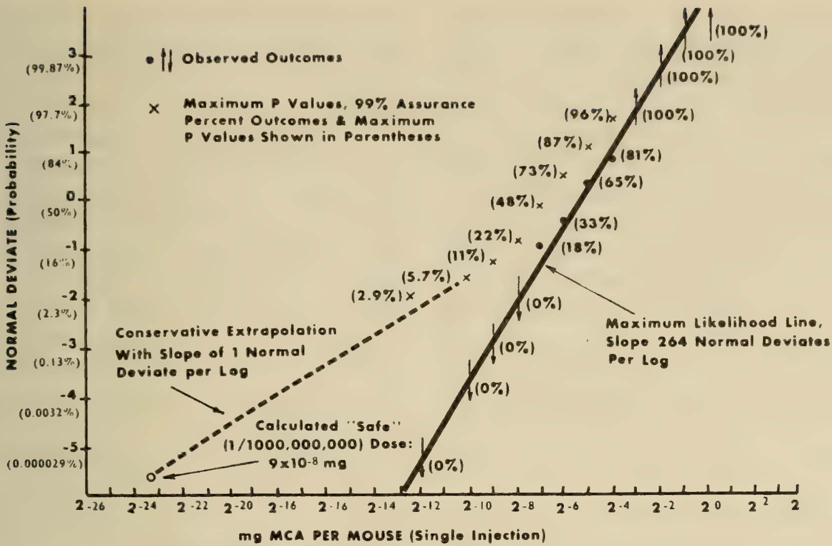

Fig. 1.—Estimation of the "safe" dose from test results with a carcinogen, methylcholanthrene, at several dose levels.

in dose. These lines are fitted at moderate to high responses, usually high experimental doses and generally using homogeneous groups of animals, which would be expected to produce steep slopes. There is no guarantee that slopes might not be less than one at low doses to which a heterogeneous human population is exposed. In fact, the dose-response in the smoking lung cancer data for man (percent of men developing lung cancer versus number of cigarettes smoked per day) gives a probit slope of about 0.75. However, a slope of one, hopefully, represents a conservative slope in the dosage range below the experimental dosages. The only notable exceptions with experimental slopes less than one which have been established are the hormonal animal feed additive growth promoters. In such cases, a slope less than one would be recommended for extrapolation.

The Mantel-Bryan procedure has these advantages: it does not require an experimental estimate of the slope; it does not require the demonstration of a statistically significant increase in tumors (which depends heavily upon the number of animals tested); it allows for a non-zero spontaneous background tumor rate; however, more research is needed where background rates are high, often resulting in treated animals with fewer tumors; it considers multiple dosage experiments. The estimated risks using the Mantel-Bryan procedure depend upon the degree of the uncertainty in the experimental data by starting the extrapolation from upper confidence limits on tumor rates and not upon proof of carcinogenicity. It does not assume that the dose-response relationship is probit log-dosage at low dosages. If a non-probit response curve is plotted on a probit scale and if its derivative is always greater than one, then the slope of one applied by the Bryan-Mantel procedure at low dosages gives a proportion of tumors which is higher than the actual proportion. However, if the same principle is applied to logistic plotting, lower extrapolated dosages will result for a given risk. It is not necessary to extrapolate to a "virtually safe" safe risk of 1 in 100 million. This value was selected by Mantel and Bryan as an "illustrative" value which probably would not be in conflict with the intent of the Delaney Clause. "Acceptable risk" is a social judgment which will have to be made by open discussions after weighing the bene-

fit of each chemical, its possible synergism with other compounds, and the uncertainty in extrapolating from animals to man.

A dichotomous procedure could be employed by extrapolating with an extremely conservative linear model for experimentally demonstrated carcinogens and extrapolating with a less conservative procedure, such as the Mantel-Bryan procedure for chemicals not demonstrated to be carcinogens. A dichotomous extrapolation rule may not encourage good experimentation to detect tumors in order that a less conservative extrapolation procedure could be used leading to higher tolerance levels.

The extreme differences between models for extrapolating to low risks have been demonstrated in Table 1. Even given a particular model, e.g. the probit, the slope was used for extrapolation produces widely different results (Table 2) where extrapolations were made from the upper 99% confidence limit.

Table 2.—Fraction of experimental dose using probit extrapolation with different slopes for an estimated risk of 1 in 100 million.

Observed tumors	Slope = 1	Slope = 1.5	Slope = 2.0
0/50	1/18,000	1/690	1/135
0/100	1/8,300	1/410	1/91
0/500	1/1,800	1/150	1/42
0/1,000	1/1,000	1/100	1/32

For example, if no tumors were observed in 100 animals, one could be 99% confident that the true risk is no more than 1 in 100 million if the dose-response curve has a slope of one probit per factor of 10 change in dose, when the experimental dosage is divided by 8300. In Table 2 it is illustrated that the current practice of taking 1/100 of an observed no-effect level for 100 animals would provide a risk of approximately 1 in 100 million if the response curve were a probit with a slope of two. However, if the slope were actually 1, then the estimated dose should be about 90 times smaller. Thus, not only is the choice of an extrapolation model critical, but the parameters used in the model, particularly the slope, are critical.

Unfortunately, one cannot verify experimentally the correct curve (model and slope) to use for extrapolation at extremely low dosages. It would be useful to obtain dose-response curves at levels lower than currently used in experiments. Perhaps a data bank can be accumulated for low dosage levels which provoke few, if any, tumors. Such data might eventually provide reasonable estimates of low dosage exposures, or perhaps, a check on the form of mathematical models. One difficulty would be differences in protocol employed by different investigators.

As was discussed by Doctors Schneiderman, Rall and Newill, we are still faced with the uncertainties in extrapolating from well-controlled animal experiments to heterogeneous human populations. Thus, there are those who rightly contend that no method of precise mathematical extrapolation exists to date. However, as I mentioned before, using 1/100 of an observed no-effect level as relatively safe is, in fact, performing an arbitrary and crude extrapolation which ignores the uncertainty in the experimental data. Predictions of tolerable dosages from animal experiments must be made. Should these predictions be made with or without the benefit of all of the scientific knowledge at hand? It is interesting to compare the Mantel-Bryan procedure with the 1/100th rule. As seen in Table 2, Mantel-Bryan would set lower levels using a slope of one if a risk of 1 in 100 million is used. Adopting an extrapolation slope of 1.5 would be in agreement with the 1/100th rule for large experiments.

An important aspect of extrapolation is the choice of the dose scale. Log dosage on a per body weight basis is frequently used as is ppm. Dosage on a surface area basis as mentioned by Doctor Rall has been investigated and appears to give a better fit to experimental data in some cases and appears useful when extra-

polating from small to large animals. No single choice can be recommended. The tendency is to express dosage in terms that give a nearly linear fit to the data in the experimental range.

Data in man, either dose-response or metabolic, may suggest greater or lesser sensitivity than the experimental animal. Human data seldom is available, but when it is available it generally is not clear how such data should be employed in a mathematical procedure for prediction of dosages producing low risks. Much more epidemiological data is needed. A current example of this is the need to use the human data from benzidine as a component of setting water effluent standards by the EPA.

Petitioners should be encouraged to conduct experiments in more than one species. Selecting the lowest tolerance for extrapolation to man from the species tested, in order to be conservative, may tend to discourage testing in several species. This appears to be the most prudent approach. Perhaps to encourage testing in more species, the slope for extrapolation could be increased as the number of species is increased. For example, if the Mantel-Bryan procedure is used in a single species, a slope of one could be used unless experimental results indicated a shallower slope. If more species were tested, steeper slopes could be allowed for extrapolation with each species, still employing the lowest tolerance from among the species tested if the experiments were done with sufficient precision that the lower confidence of the slope could be determined statistically with high confidence to be greater than the slope to be used. For example, an experimental slope of 4 with a lower boundary of 3 might allow for using 1.5 rather than 1. This procedure is only a suggestion which should be investigated with existing data to determine its workability.

Another important aspect of extrapolation is determining the level of an acceptable risk. This is a social-political decision which cannot be made by the scientist alone, but requires a risk-benefit analysis with input by many segments of society. This is an awesome task, but we are faced with it daily in setting speed limits, building codes, etc. Admittedly, it may be easier to perform risk-benefit analyses for many aspects of life than for food additives. Few, if any persons, would want a potential carcinogen added to the food supply if its only benefit were esthetic. There would be no need for extrapolation to a tolerable dosage and the Delaney Clause should remain unchanged. However, if an additive is a preservative which prevents or lessens the risks of other diseases, a nutrient which may reduce the risk of other diseases, or improve nutrition by making food less expensive, then a risk-benefit analysis may be in order. For comparative purposes, dose reduction factors for a risk of 1 in a million are given in Table 3 using a probit slope of 1. Again, with a large number of animals, the 1/100th dosage of an experimental no-effect level is not too different.

Table 3.—Fraction of experimental dose using probit extrapolation with a slope of one for an estimated risk of one in a million.

Observed tumors	Fraction of experimental dosage
0/50	1/2,500
0/100	1/1,140
0/500	1/250
0/1,000	1/140

If the extrapolations were correct what does a risk of 1 in 100 million for a lethal tumor really mean? Approximately 1/6 of the people in the United States eventually die due to cancer. An additional 1 in a 100 million would be unnoticeable. Mantel and Bryan suggested that a calculated risk of 1 in 100 million is the practical equivalent of 0 since the conservative procedure used, if correct, sets 1 in 100 million as the *upper* limit on the true risk. The Mantel-Bryan procedure does not attempt to accept a risk of 1 in 100 million but is directed toward a zero risk not exceeding 1 in 100 million.

Table 4.—Probability of tumor incidence estimated using Mantel and Bryan.

Benzo (a) pyrene mg/kg/man/day	Probit slope 1.0	Probit slope 1.5
.010	1×10^{-6}	1×10^{-14}
.020	1×10^{-5}	1×10^{-12}
.040	2×10^{-5}	1×10^{-11}

Some estimated risks are calculated by Friedman (1973) (Table 4) using the Mantel-Bryan procedure based on a mouse intubation study for benzo(a) pyrene by Berenblum and Haran (1955). Depending on daily intake at human exposure levels, estimated risks range from 2×10^{-5} to 10^{-6}, depending heavily upon the slope used. These data illustrate that we already may be accepting what some people would regard as a relatively high risk, 2 in 100,000, in our food supply from charcoal broiled meat. Of course, the individual can make a choice in this instance. Such information, which is often meager and tentative, may not be available or meaningful.

Experimental Design

In testing for carcinogenicity, it is not clear that current experimental designs and methods of analysis are the best that can be developed. It is difficult to detect and estimate the dose for even a high risk, say .01, when the spontaneous background rate is high, say 0.10. However, it may not be desirable to choose a strain of a species of animals with a 0 or near-0 spontaneous rate, as that strain may be resistant to the chemical. It may be desirable to consider relative rather than absolute rates.

The choice of responses to analyze (e.g., proportion of animals with tumors, number of tumors per animal, or time to tumor) will dictate the experimental design. Consideration must be given to the range of dosages, number of dosages, number of animals, length of feeding (total dose), and times of sacrifice, if any.

If a procedure such as the Mantel-Bryan procedure were adopted for extrapolation, it is possible to calculate "acceptable dosages" for given risks as a function of the proportion of the experimental animals producing tumors (which may be zero) and the number of animals employed.

Considerably more research is needed in the development of experimental protocol for predicting carcinogenicity of chemicals, and I feel the NCTR will impact heavily on this area.

Time to Tumor Occurence

A risk of 1 in 100 million represents an additional two people, now living in the United States, who would eventually die of cancer rather than due to some other cause. This raises an important and difficult question. Since cancer generally occurs late in life, would these two cases result in the loss of life of a few days, weeks, or years? Murray and Axtell (1973) investigated this question. The question of time to tumor occurrence becomes critical. Extrapolating to low dosages from a classical dose-response curve does not consider the time at which tumors occur. The experimental response may be the gross percentage of tumors occurring over the lifetime or up until the termination date of an experiment. One recognized difficulty with gross rates is that the animals at the highest doses may have shorter life spans due to toxicity of the chemical and therefore have less opportunity to develop tumors. Thus, the highest dosage may exhibit the lowest gross proportion of tumors. Some, but not all, researchers have made adjustments for changes in mortality due to competing causes of death. The simplest device which has been employed is to sacrifice animals at a fixed point in time (e.g., 18 months with mice) and to observe the percentages of those animals possessing particular tumors. This gives the proportion with tumors to a given time which can be plotted against dose. As an attempt to study time to tumor development, serial sacrifice experiments have been employed with scheduled sacrifices at several points during the life span of the animals. Such

experiments, require a large number of animals. A procedure such as Mantel-Bryan could be used for extrapolation at each time of sacrifice. Now a difficult question arises, does one use the highest tolerable dose found at different sacrifice times or should more weight be given to the earlier tumors? If more weight should be given to early tumors, how much? Also, a great deal of information is generally lost due to animals that die before the termination date or between serial sacrifice times of an experiment. In fact, it may be these animals which die early that contain the most important information because they afford an opportunity to observe tumors early in life.

Time Concept

I hope the highlighting of the work of Blum, at the beginning of this discussion, indicates that we in chemical toxicology are not completely ignorant of the contributions made in radiation as was suggested by one observer.

Blum, in his 1959 work on "Carcinogenesis by Ultraviolet Light", demonstrated a log normal distribution of exposure and cancer development time. (Princeton University Press, Princeton, New Jersey (1959)). It was Druckrey however who dramatized the relationships of time, dose, and dose rate for chemicals in a 1967 monograph representing 25 years work and 10,000 experimental animals. (Quantitative Aspects in Chemical Carcinogenesis, U.I.C.C. Monograph No. 7, Potential Carcinogenic Hazards from Drugs, pp. 60–77 (1967)).

Fig. 2 was chosen because it demonstrates that over a considerable dose range there is not an experimentally observable no effect or threshold dose for diethylnitrosamine. Mr. Wands asked questions of risk benefit and used nitrosamines as an example. Also this figure is the work of Druckrey on which much of which I will speak is based. Can we further quantitate risk?

Druckrey emphasized that if proper scientific judgments are to be made re-

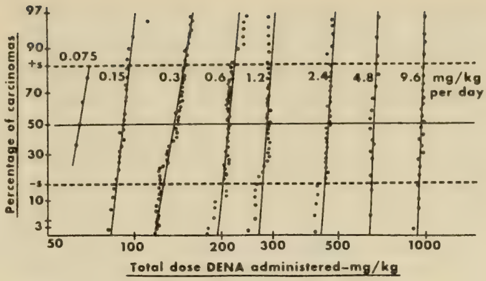

Fig. 2.—Dose-response relationships for the carcinogenic action of diethylnitrosamine (DENA) in BD II rats.

garding risks from carcinogens, knowledge of the pharmacological relationships, especially dose-response relationships, must be established. He pointed out that thorough investigations of these fundamental problems are only possible in systematic, highly controlled, mutually comparable animal experiments. He further pointed out that true advances can only be expected from quantitative results that are expressible in measurement and number and are available for criticism.

Druckrey first demonstrated with 4-dimethyl-amino-benzene the dependancy of time and dose. According to the relationship developed, the product of daily dosage and induction time is a constant ($k = dt$). He observed that the same carcinogenic response was obtained for smaller dose rates and total doses if time was extended ($k = dt^n$). These results, and others, suggested that primary carcinogenic effects for 4-DAB remain irreversible over a whole lifespan and suggested the appropriateness of the concept of "*Summation Action*" which he had introduced 20 years earlier.

Druckrey extended his observations to 4-dimethyl-amino-stilbene (DAST) as illustrated in Fig. 3.

The plot of percentages of tumors expressed as probits vs. log total dose of 4-dimethyl-amino-stilbene (Druckrey, Schmahl, and Dischler, 1963), demonstrates a parallel and linear relationship between probits of ear duct and mammary carcinoma and log total dose. This clearly demonstrates that for 4-DAST

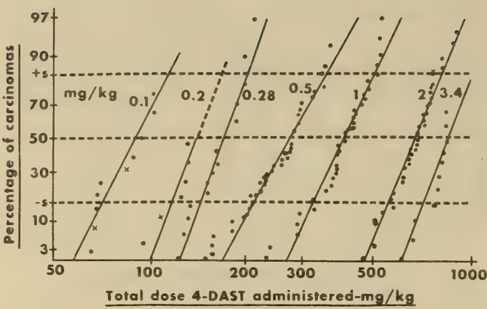

Fig. 3.—Incidence of carcinomas is dependent on the sum of doses, 4-dimethylamino-stilbene.

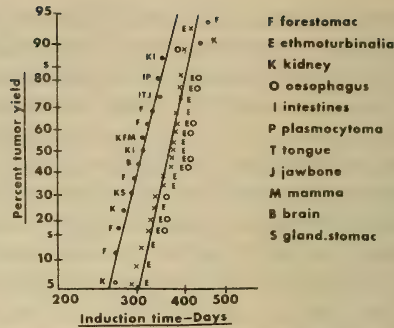

Fig. 6.—Normal distribution of the induction times in carcinogenesis, dependent upon the variety of organs of tumor development in BD rats.

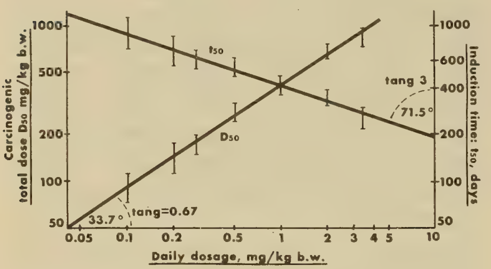

Fig. 4.—Linear dependence of the median carcinogenic total dose D_{50} and of the median induction time T_{50} on the daily dosage of 4-dimethylamino-stilbene.

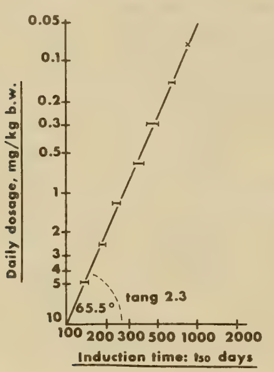

Fig. 5.—Linear dependency of the median induction times (T_{50}) upon the daily dosages of diethylnitrosamine.

the total dose required to produce a given incidence is clearly smaller at smaller dose rates.

A replot of log total dose, and median induction time, vs. log dose rate also demonstrates a linear relationship between the log median induction time and log dose rate (Fig. 4).

This plot identifies the limiting component lifespan. This figure illustrates for a replot of Druckrey's data, that time begins to be limiting as an experimental variable, a fact that toxicologists and statisticians must work together to exploit. Doctor Schneiderman's comments of a 300 year component in cigarette smoking however demonstrates the complexity of the concept. The difficulty in observing this is not great but attention should be placed on the lack of evidence or suggestion of a sub-threshold dose (Fig. 5). For this specific example, $n = 3$ and $k = dt^3$.

Surprising to me, but as you can see from Fig. 6, the linear relationship held for a number of organ and tumor types generated with methyl nitrosourea and n-nitroso piperidine.

What data exists for man? Doll advanced the concepts relating time and dose still further in his 1970 paper read before the Royal Statistical Society (The Age Distribution of Cancer: Implications for Models of Carcinogenesis. *J. Royal Stat. Soc.* 134 (2): 133–166 (1971)), where he proposed the $I_t = b\ (t\text{-}v\text{-}w)^k$ where v is time of beginning exposure, and w is the minimum time for clinical recognition for a tumor. Doll applied this concept to data on the incidence of bronchial carcinoma in cigarette smokers.

Doll examined data reported by Day in 1967 on tumors from tar painted on the skin of mice. The n obtained for cigarette smoking in man and skin tumors in mice were strikingly similar and encouraging to those that are supportive of efforts to describe mathematically, similarities in cancer responses.

Doll points out that a wide range of pairs of values for k and w in the formula $I_t = b\,(t\text{-}v\text{-}w)^k$ would fit the data and that a better approach would be to design protocols to estimate the values independently. This is an important point that the carcinogenesis protocol at NCTR addresses. Doll further pointed out that examination of skin cancer generated by benzo (a) pyrene would fit $b\alpha$ dose^2 better than $b\alpha$ dose.

There has been some more recent research in statistical techniques to analyze time to tumor data. One useful measure of the impact of carcinogenesis on a population may be age-specific incidence rates or the amount of life shortening due to a tumor. Time to tumor data requires life time studies to estimate a relationship between tumor rates, time, and dosage. This is followed by extrapolations to low dosages which may project time to tumor development beyond the normal lifespan of an animal. Then, for a given dosage a time to tumor distribution must be employed to estimate the proportion of animals expected to develop tumors within their lifespan before dying of other causes. The approach is quite complicated and depends on mathematical assumptions which need to be checked experimentally (the statistical distribution of time to tumors and their relationships with dosage) for many different types of chemicals, tumors, and experimental animals. Such experiments require survival studies and several dosages employing large numbers of animals. Again, there does not appear to be sufficient evidence at this time to recommend specific procedures. It is important to obtain dosage rate effects on the time pattern of response, especially to determine the extent it influences incidence rate and to the extent it influences time to tumors at all incidence levels.

Albert and Altshuler (1973) have developed a mathematical model for predicting tumor incidence and life shortening based on the work of Blum (1959) on skin tumor response with chronic ultraviolet irradiation in mice and on Druckrey (1967) for a variety of chemical carcinogens in rodents. Albert and Altshuler have investigated radiation cancer in mice exposed to radium and also to cigarette smokers. In review, the basic relationship used is: $dt^n = c$, where d is dosage, t is the median time to occurrence of tumors, n is a parameter greater than one, and c is a constant depending on the given experimental conditions. It is of interest to determine the time it takes for a small proportion of the population to develop tumors. With this formulation, as the dosage is increased, the time to tumor occurrence is shortened. Albert and Altshuler use the log-normal distribution to represent time to tumor occurrence assuming the standard deviation to be independent of dosage.

Dr. Nancy Mann, in addition to lively introductions and elevation of the esthetic level of the speakers' platform, discussed the Weibull distribution. The Weibull distribution for time to tumors has been suggested by human cancers, (Cook, Doll, and Fellingham 1969; Lee and O'Neill, 1971); $I = bd^m\,(t\text{-}w)^k$, where I is the incidence rate of tumors at time t, b is a constant depending on experimental conditions, d is dosage, w (the minimum time to the occurrence of an observable tumor), m and k are parameters to be estimated. Also, Day (1967), Peto, Lee and Paige (1972) and Peto and Lee (1973) have considered the Weibull distribution for time to tumor occurrence. Theoretical models of carcinogenesis also predict the Weibull distribution (Pike, 1966). Theoretical arguments and some experimental data suggest the Weibull distribution where tumor incidence is a polynomial in dose times a function of age.

The log-normal distribution of tumor times corresponds to the probit transformation as employed in the Mantel-Bryan procedure. Use of the Weibull distribution for time to tumor leads to an extreme value distribution relating tumor response to dosage (Chand and Hoel, 1973). Hoel (1972) gives techniques when adjustments must be made for competing causes of death. Albert and Altshuler (1973) discuss other distributions of time to tumor. What must be done is to encourage more experimentation and statistical research on survival studies. It probably would be much more palatable to set safe doses on the basis that the probability of a tumor is remote if an animal lived to, say, twice its normal lifespan; rather than to say that the probability is remote that an animal develops a tumor during its lifespan. However, it is still the latter quantity which is of concern. We have invited Doctors Albert and Altshuler to examine the data bases at NCTR in hope of increasing the documentation of their model in large carefully controlled animal experiments.

Variation in Exposure

I am borrowing Doctor Newill's slide to demonstrate variation in exposure. Human intake of a chemical varies among individuals and varies daily for a given individual. The simplest approach and perhaps adequate for our current state of knowledge is to calculate risks for anticipated "maximum" exposure levels. This gives additional conservatism for any prediction technique. Some contend that it is not necessary to attempt to protect every last individual with unusual habits, but to base predictions on average intake, for example, in an attempt to estimate the actual risk to the population.

Mathematically, the proper approach to calculate the risk for the total population is to calculate the risk for a given dosage (a most difficult task as discussed in the previous sections) and then to multiply that risk by the proportion of the time that dosage occurs followed by integration over the distribution of dosages. Generally, the distribution of dosages for an environmental chemical in a human population would be unknown. Thus, introducing variation in consumption adds another dimension to be investigated to an already complicated problem. This, in effect, gives the average risk and does not consider a segment of the population which may be at high risk. Indeed the lively discussion from the floor about NO_X levels in a kitchen speaks to this problem.

I wish to share with you an outline of one of the chronic dose-response studies being conducted with 2-AAF, a carcinogen, at NCTR. I feel that an introduction into the needs of FDA and EPA may be of value in understanding our approach.

I. Responsibilities

A. *Food and Drug Administration.*— The general public may be exposed to chemical carcinogens from the food it consumes, the drugs it uses, and the cosmetics it enjoys. It is the responsibility of the FDA to determine the risk involved in the use of these commodities by the general public. However, it must be recognized that there is no way to guarantee absolute safety. Small populations of experimental subjects, either animal or man, provide an imprecise basis for comparison to a large human population of variable genetic and disease states, cultural backgrounds, and ages. Furthermore, toxicological assessments are made on individual chemicals and humans are exposed to a milieu of interacting chemicals. Leading us somewhat out of this apparently chaotic situation, studies with laboratory animals have shown that nearly all chemicals that are carcinogenic in man are also carcinogenic in one or more animal species although the tumors may be of a different type. Thus, the carcinogenic properties of a chemical may be detected in experimental animals just as we detect the life shortening or lethal properties of a chemical. It is apparent that, while cancer testing in ani-

mals in terms of "an all-or-none effect" has reached a sophisticated level of development, the area of quantitative dose-response relationship for testing of chemical carcinogens needs much more in-depth study. It is precisely this area that must develop if a rational assessment is to be made of "acceptable risk" and "acceptable daily intake" of a chemical carcinogen in our food, drugs, or cosmetics.

B. *Environmental Protection Agency.* —An understanding of why certain environmental agents produce adverse effects and the circumstances that determine the severity of these effects is the basis of all environmental health regulatory control. Carcinogenic substances pose a hazard to man and the environment through several distinct pathways. The most obvious of these is direct ingestion. Control of chemical food additives is the responsibility of the Food and Drug Administration as indicated earlier. However, the presence of unintentional residues in food such as residues of pesticides and other toxic substance is a responsibility of the Environmental Protection Agency. This responsibility extends to cover chemicals present in water and air and their effects not only on man but to any component of the environment.

In terms of man, it is clear that the main chemical carcinogenic hazards result from exposure associated with food, water, and air. The degree of the hazards involved depends on many factors. One of the basic factors involves the quantitative aspects of a chemical carcinogenic action. Although the many factors involved are important, it is the quantitative aspects which has not received intensive study and which is basic to determining acceptable daily exposure levels for a chemical carcinogen. These acceptable daily exposure levels, in turn, are a basic requirement to setting standards for chemical carcinogens in our food and our environment.

C. *Others.*—The National Institute of Occupational Safety and Health (NIOSH) of the Department of Health, Education, and Welfare and the Occupational Safety and Health Administration (OSHA) of the Department of Labor share the regulatory responsibilities for the control of hazards in the occupational environment. These responsibilities involve the control of hazards associated with occupational exposure to chemical carcinogens. Thus, the concepts methodology involved in evaluating quantitatively the risk involved in exposure to these substances is a basic need of these agencies.

Finally, it must be recognized that the necessity of information on the quantitative determination of acceptable exposure levels to carcinogens is basic not only to the regulatory agencies, but is of particular importance to those involved in meeting our extensive chemical needs. Chemicals as part of various products, drugs, pesticides, food additives, water additives, etc., are a necessity of life and, in turn, create a necessity of information permitting their use within acceptable limits of risk.

II. *Deficiencies in Work to Date and Factors to be Considered in Protocol Development.*—Most of the deficiencies in carcinogenic testing result mainly from the concept that this testing involves only the determination as to whether or not a compound can be made to produce a neoplastic tumor. However, it is now recognized that carcinogenic testing must, of necessity, consider both qualitative and quantitative factors. The main deficiencies in past studies of these factors involve primarily two areas, i.e., experimental design and definition of end points.

A. *Experimental Design:*

1. *Statistics.*—The bulk of the technical literature reflects the lack of statistically valid experimental design including adequate numbers of animals at low levels of carcinogenic response.

2. *Dose Response.*—Only limited use of dose response studies are reported

in the technical literature for the purpose of determining tumor incidence and time to tumor in terms of dose rate and total dose. The prediction of risk at a given exposure level requires dose response information.

3. *Low-Dose Studies.*—Little information is available on dose-response studies at low levels of exposure and response. At low levels of exposure environmental factors may alter extensively the quantitative aspects of a response.

4. *Mathematical Models.*—There is only limited mathematical definition of the dose-response curves at low levels of exposure in terms of variables affecting a chemical carcinogenic response.

5. *Life-Shortening.*—There is limited use of experimental designs which permit proper observation and evaluation of life-shortening effects of chemical carcinogens in relation to dosage.

6. *Age Sensitivity.*—The hazards involved in exposure to a chemical carcinogen depend not only on the nature of the chemical itself, the route of exposure, and the extent of exposure in terms of amount of time, but also on the susceptibility of the animal at the time of exposure. There are only limited studies available on the influence of age on the sensitivity of an animal to a chemical carcinogen.

7. *Recovery.*—There is a lack of evaluation of the possible regression or progression of pretumorous lesions such as hyperplasia in relation to dosage.

8. *Tumor Growth Rate.*—The technical literature shows an impressive lack of study of the possible dependency of tumor growth rate on dosage.

9. *Reproducibility of Results.*—There is an extensive lack of evaluation of the quantitative reproducibility of chemical carcinogenic testing.

B. *Endpoints:*

1. *Tumorigenesis.*—Tumorigenesis is as important an endpoint as carcinogenesis. Benign tumors may cause death in man and animals without ever undergoing malignant transformation. There can be no doubt from a survey of the technical literature that benign neoplasms are often precursors of malignancies. In the light of present knowledge, all tumorigens must be regarded as potential carcinogens. Hyperplasia and number, type, grade, and individual distribution of tumors must all be carefully used as endpoints in the evaluation of chemical carcinogenesis.

2. *Time to Tumors.*—In some cases the only manifestation of an effect consists of an earlier occurrence of tumors in the treated animals than in the controls. Time to tumor may be a very sensitive endpoint permitting estimation of "acceptable exposure levels" from dose-time to tumor curves. This endpoint in chemical carcinogen testing merits further in-depth study.

3. *Life Shortening.*—As indicated earlier, there is limited use of experimental design which permit proper observations and evaluation of life-shortening effects of chemical carcinogens in relation to dosage.

4. *Pathology.*—It is of the utmost importance that a complete and accurate pathological examination be conducted on all animals used in carcinogenesis studies. There is no doubt that benign tumors may cause death without undergoing malignant transformation. All lesions, including precancerous lesions such as hyperplasia, must be described. Number, type, grade, and individual distribution of tumors must all be carefully evaluated in a chemical carcinogenesis study. The lack of proper pathological capabilities often limits this most critical aspect of such a study.

5. *Biochemistry.*—The evaluation of carcinogenic hazards for man is based on a judgment of all available information. That is, it is based not only on the carcinogenic bioassay, toxicity tests, epidemiological data, and on the extent and route of exposure of man, but also on

metabolic, biochemical, and pharmacokinetic studies. Each compound must be evaluated individually on the nature of its absorption, distribution, metabolism, retention, and excretion.

Ancillary support experiments, generally independent of the large ED_{01} Barrier Study, will be undertaken within the programs of the Divisions of Chemistry and Comparative Pharmacology. The purpose of these studies will be to define appropriate biochemical endpoints and the role of pharmacokinetics to aid in the evaluation and interpretation of the large carcinogenic bioassay. For the most part, these studies will be undertaken with mice not maintained in the barrier experiment. A select number of biochemical parameters, however, can be measured in some mice at the time that these animals are removed from the ED_{01} experiment.

Biochemical endpoints, as an indicator of or response to carcinogen exposures, are not usually included in carcinogenic bioassays. Identification of reliable indices that relate directly to tumorigenesis would be invaluable to possibly define susceptible or non-susceptible individuals in an animal population or to possibly determine the time to onset of irreversible lesions during a precancerous induction period. This concept is highly important to and related to the chronic low dose carcinogenic bioassays. However, the current status of this concept has not been definitely proven or confirmed and, as such, must be considered as an activity peripheral to the large bioassay study at this time.

Logically, any stimulus such as a chemical carcinogen producing an anabolic or precancerous change in a tissue such as liver should produce some response, such as stimulation or inhibition of an enzyme(s) that can be detected biochemically in the affected tissue or possibly in the blood. The inherent problem is to select or find the proper biochemical endpoint. Several prospective endpoints have been defined, but their potential utility as predictors or indicators of response remains to be established. Research activities in some of these biochemical indicators are centered in the Divisions of Chemistry and Comparative Pharmacology.

The role of DNA repair in the toxic and carcinogenic effect of 2-AAF also must be considered in relation to the chronic low dose bioassay. Recent evidence resulting from studies on the effect of radiation on biological systems indicates that mammalian cells have the capability of repairing damage to their DNA. More recently it has been demonstrated that many chemicals such as AAF form covalent bonds with DNA and are removed by a process of "unscheduled DNA synthesis" or DNA "repair synthesis". The process appears to involve the excision of the damaged segment of DNA with concomitant replacement by repair synthesis. The importance of this process was made evident with the demonstration that the resistance of numerous tumors to chemotherapeutic agents could be correlated with their level of DNA repair activity. Tumors resistant to chemotherapeutic agents were found to be susceptible in the presence of DNA repair inhibitors such as caffeine or chloroquine.

It is often assumed that DNA repair always acts in a protective way by removing damaged DNA segments or bound chemical residues. It is known, however, that the probability of an error in DNA replication which might result in a mutation increases with the extent of DNA synthesis. The possibility of AAF producing mutations in DNA by stimulating extensive DNA repair synthesis is a real one and must be considered in any study concerning the role of DNA repair in carcinogenesis. In any event, a more complete understanding of how a cell repairs the damage inflicted upon its genetic information by chemicals in general and carcinogens in particular will be necessary. An understanding of the role of DNA repair in carcinogenesis is basic to the question of whether small chemical insults to a cell are completely repaired or accumulate over a long period of chronic exposure.

A study to investigate the role of DNA repair in the carcinogenic process has been initiated within the Division of Comparative Pharmacology. The specific objectives of this study are to seek a correlation between DNA repair and tumorigenesis under several experimental conditions, to determine the effects of acute and chronic doses of 2-AAF on DNA repair, to evaluate the effect of DNA repair inhibitors on tumor incidence, and to investigate the possible interrelationships between DNA repair and cell division in carcinogenesis. Results from this study will provide valuable input to the understanding and interpretation of the chronic study with 2-AAF.

6. *Pharmacokinetics.*—In order to provide a firmer basis for evaluation of results obtained in the large chronic low-dose carcinogenic bioassay, it will be essential to develop a correlation between dietary level of the carcinogen, total and/or daily intake of chemical, incorporation of chemical into the target site (bladder in this instance) and the incidence of bladder tumors as a function of duration and level of exposure. Involved also in this correlation is the need to evaluate the role of blood levels (total as well as unbound) and urinary excretion patterns of the chemical and/or its metabolites. The overall concept or process described is the basis and definition of pharmacokinetics. Pharmacokinetics basically measures rates of chemical absorption, distribution, tissue binding and storage, metabolism, and elimination. Elimination in this case meaning excretion through urine, feces, and expired air. Mathematical models are designed to analyze results by means of computer simulation.

With 2-AAF, a unique opportunity is presented to relate dietary levels and feed consumpation to relative levels of the compound or metabolites in blood, urine, urinary bladder, and incidence of bladder tumors. The key comparisons will have to evolve based on chronic exposure of the animal to the test compound. However, to develop a model on which to evaluate results from chronic exposure, it was necessary to undertake a series of acute and subacute experiments designed to determine as a function of dose level the absorption, distribution, metabolism, excretion, and bladder binding of 2-AAF following single and multiple P.O., I.P., and I.V. doses of chemical as well as following dietary exposure. Based on models developed from these studies, responses were predicted for chronic exposure to 2-AAF; the accuracy of these predictions will be verified from results that will be obtained in a chronic exposure metabolism study. In terms of the large ED_{01}-2-AAF study, this approach will be limited to establishing dosage levels of 2-AAF to concentrations of the compound and/or its metabolites in blood with reference to time and to the effect on the endpoint being studied.

The more in-depth pharmacokinetic study will be undertaken within the Division of Comparative Pharmacology as a separate study from the large ED_{01}— 2-AAF experiment.

III. *Approaches.*—It is clear that human exposure to many chemical carcinogens is inevitable at the present time and in the foreseeable future. It follows that a need exists for capabilities which would permit an evaluation of the relative hazards posed by different chemical carcinogens. The development of methodology for adequately evaluating carcinogenic risk involves two major approaches. The first is the establishment of a carcinogen dose-response relationship using various endpoints such as tumor prevalence, time to tumor, life shortening, etc. This carcinogen dose-response relationship must permit some mathematical extrapolation downward on the curve so as to facilitate determination of risk at levels of realistic exposure. These are the primary objectives of this study. The second approach which is beyond the scope of this study is to develop methodology and concepts which will permit extrapolation of results to man.

A. *Dose Response.*—The dose-response of tumor prevalence in terms of dose rate and total dose, giving appropriate consideration to cause of death, will be determined. Such data should rival the best mathematical model for the conservative extrapolation from dose-tumor prevalence curves to an exposure level that would pose a "socially acceptable risk".

B. *Time to Tumor.*—There is increasing interest in the time to tumor dose-response relationship in chronic studies. Early tumors have much more impact on lifespan than do late tumors. Furthermore, for some carcinogens, in particular at low levels of exposure, the only manifestation of an effect consists of an earlier occurrence of tumors in the treated animals than in the controls, the tumor prevalence being the same in both. The prevalance of tumors as a function of age (time to tumor) over the life span of the animals provides a better description of the tumorigenic process than at a single point in time of sacrifice or the total prevalence of tumors over the life span. Two problems need to be resolved. First, the relationship between dosage and median time to tumor must be established. Secondly, given the dosage, the distribution of time to tumors must be established to estimate the prevalence of tumors during the life span for a given dosage. The survival group (life span group) in the experimental design of this protocol will provide good data for such analyses.

C. *Life Shortening.*—The lifespan portion of the experimental design allows the evaluation of life shortening as an endpoint for chronic studies. All of the information gathered on time to tumor development also demonstrate life shortening for lethal tumors.

D. *Age Sensitivity.*—Hazards involved in exposure to a chemical carcinogen depend not only on the nature of the chemical itself, the route of exposure, and the extent of exposure in terms of amount and time, but also on the susceptibility of the animals at the time of exposure. Age sensitivity studies are being conducted in a separate experiment in order to permit evaluation of possible age sensitivity to 2-AAF in relation to specific periods of treatment of mice with this compound during serial sacrifice and serial treatment phase of the ED_{01}—2-AAF Study.

E. *Regression or Progression of Effects.*—The experimental design permits groups fed 2-AAF for 6, 9, and 12 months and sacrificed to be compared with groups fed the same length of time but sacrificed at 18 months. The purpose is to study the possible regression or progression of pretumorous lesions such as hyperplasia in relation to dosage and time.

F. *Tumor Growth Rate.*—The experimental design permits an approach to the question of the possible dependence of tumor growth rate on dosage and treatment as will be revealed in the serial sacrifice and serial treatment phases of the study.

IV. *Experimental Design.*—The design contains both basic types of experiments: survival (lifespan) and serial sacrifice. The serial sacrifice portion is sub-divided into continuous and discontinued feeding.

A. *Pilot Studies.*—A pilot study in which 2-AAF was administered in the feed of mice for eighteen months established the suitability of the strain and sex and gave indications of the dosage-time range to be used in the more extensive ED_{01} Study. The results of the pilot study will be published elsewhere.

B. *Animals*

1. *Species, Strain, and Sex.*—Mice were selected for this study because of the need for large numbers of animals required for the statistical validity of dose-response studies at low levels of exposure (dosage) to a chemical carcinogen. The choice of mice is further substantiated if one considers the availability of well defined inbred strains of animals having a relatively short life span.

The BALB/c strain was selected because of the lack of spontaneous bladder tumors in contrast to its high susceptibility to 2-AAF induction of these tumors. Based on general concepts and on Pilot Study results, the main objective, that is the development of suitable mathematical description of a chemical carcinogen dose-response curve permitting extrapolation from high to low levels of response, was determined to be equally possible with either sex of the selected strain. The dosage range studied in the pilot experiment gave better data points on the curve for the female than for the male BALB/c mice, therefore, females were selected for the ED_{01} Study. The nature of the dose-response curve at low levels of prolonged exposure to a chemical carcinogen could be studied using both sexes of several strains of mice and using several carcinogens and types of tumors. Such an extensive experiment would be considered best after a limited and more circumscribed study revealed the need, amplified the approach and indicated the success and usefulness of such an undertaking.

2. *Age of Animals.*—All animals allocated to the experiment will be weanlings, three to four weeks of age.

3. *SPF-DF Animals.*—All mice used in the experiment will be "specific pathogen free defined flora" animals derived in the breeding colony of NCTR from a Charles River substrain of BALB/c mice.

C. *Dosages.*—Based on the Pilot Study results, seven dosages expressed as ppm of 2-AAF in the feed were selected to give approximately a tumor prevalence of 64 through 1% as indicated in Table 5. It must be recognized that the dose-response relationship expressed in Table 5 is based on an 18-month study in which the animals used were not SPF-DF animals maintained under barrier conditions. Furthermore, although the mice were BALB/c females, they were from a commercial source and were not derived from the NCTR mice breeding colony. It must be stressed that the dose response relationship is considered the best approximation which could be made when all the available data were considered. The accuracy of this approximation can only be determined by the ED_{01}-2-AAF Chronic Study.

Table 5.—Bladder tumor prevalence with 2-AAF in feed.

2-AAF Concentration in Feed (ppm)	Bladder Tumor Prevalence (ED%)
200	64
175	32
150	16
100	8
50	4
25	2
10	1

D. *Type and Duration of Treatments.*—The survival phase of the study involves a lifespan exposure to 2-AAF in the feed. The animals in this phase of the study will be removed from the experiment as they become moribund. The serial sacrifice phase involves treatment for and sacrifice at 6, 7, 8, 9, 12, 15, and 18 months. The serial treatment or recovery study involves treatment for 6, 9, and 12 months followed by recovery and sacrifice at the eighteenth month of entering the experiment.

E. *Statistics.*

1. *Grouping and Randomization of Animals.*—As animals are received from Animal Husbandry Division, they will be randomly allocated to the various experimental groups. This will insure that any differences in animals, feed, laboratory conditions, or handling will be approximately the same for all experimental groups. For ease of operation, treatments will be grouped in tiers of six cages on a rack. This will also average out floor-to-ceiling differences in temperature, light, and humidity if these should be important factors.

2. *Sequential Entry.*—The animals will be placed on experiment, a room at a

time. Each barrier room pertinent to the ED_{01}-2-AAF Chronic Study will be loaded at the rate of two racks per week, requiring seven weeks to load a room. The randomization of animals to treatments described above should nearly eliminate most changes that occur with time.

3. *Replication of Module.*—To conduct the experiment, the module presented in the experimental design will be replicated six times. That is, the experiment will be conducted in six barrier rooms. This should provide adequate numbers of animals to estimate dose-response slopes within ±50% and to estimate the ED_{01} levels within a factor of two. Thus, mathematical models that differ by a factor of three at the ED_{01} levels can be detected.

Table 6 presents the number of different dose groups and the number of different experimental components in each of the six rooms.

Summary

Many facets of life, including food products currently consumed involve risks. It is a worthy goal to strive for absolute safety, but it is impossible to demonstrate absolute safety experimentally.

The problem is not to determine whether or not a socially necessary compound is a carcinogen at high experimental doses, but to estimate risks at low dosages approximating human exposure levels. Such estimation procedures should require the setting of tolerances based on the certainty of experimental results.

In order to observe biological effects with adequate statistical precision from a reasonable number of animals, experimental dosages are generally well above human exposure levels. Thus, extrapolation of effects to lower dosages must be made to estimate risks.

Estimated risks vary widely depending on the mathematical model used for extrapolation and the values of the parameters used in the model.

Age specific tumor rates may give an incomplete description of the tumorigenic process. More emphasis is needed on survival studies in which time to tumor occurrence is studied. A parameter such as life shortening may be more meaningful than the proportion of animals developing tumors.

More information is needed on comparisons of results for various chemicals and species from survival studies, including the effect of dosage on the param-

Table 6.—2-AAF Chronic Study (Cages/Room).

Purpose	Survival	Serial Sacrifice							Serial Treatment			Diagnostic	Total
Mo. of Sacrifice	None	18	15	12	9	8	7	6	18	18	18		
Mo. on 2-AAF	Life	0-18	0-15	0-12	0-9	0-8	0-7	0-6	0-12	0-9	0-6		
ED_{64}	6	12	6	6	6	6	6	6	6	6	6	6	78
ED_{32}	6	12	6	6	6	6	6	6	6	6	6		72
ED_{16}	12	24	12	12	6	6	6	6	12	12	12		120
ED_{8}	12	24	18	18	12	12	12	12	18	18	18		174
ED_{4}	18	36	24	18									96
ED_{2}	36	72	36										144
ED_{1}	72	144											216
ED_{0}	18	36	12	12	6	6	6	6				6	108
Total	180	360	114	72	36	36	36	36	42	42	42	42	1008

Months denoted as time on treatment.
Dosages based on expected % of bladder tumors at 18 months.
Four mice per cage; 72 cages per rack; 14 racks per room.
Repeat experiment in 6 rooms for a total of 24,192 animals.
Replace cages for diagnostics as animals are depleted after 6 month serial sacrifice.
Use BALB/c female weanling mice.

eters of the time pattern response both in man and animals.

More information is needed on human intake of various chemicals so that estimates of risk can take into account variation in exposure rather than calculating risks for average or "maximum" consumption.

I would be remiss if I did not stress that the major advantage of animal toxicology over human epidemiology is that the toxicity can be predicted *before* human exposure.

I hope that I have adequately dealt with some of the questions from the floor and discussed some of the needs in statistics from the vantage point of a toxicologist. I have attempted to identify one of the scientific programs at NCTR, the chronic low level dose response experiment with 2-AAF.

I must emphasize that there are two other areas in toxicology equally needy in basic dose response experimentation: mutagenesis and teratogenesis. The NCTR is launching programs in these areas equal in depth to the chronic study I have described. In closing let me take a few minutes of your time to discuss why we cannot make decisions as to the adverse health of any chemical in a vacuum. We are now facing a severe fossil fuel energy shortage. Most of the world is facing a severe nutritional shortage. Consider the following chain of events. The United States exports grain and improves our balance of payments. The United States imports oil and shifts our balance of payments toward a deficit. The EPA wishes to improve air quality and among several approaches is the use of low sulfur fuel and control technology. Both cost money. An energy crunch comes along with escalating costs. The FDA, also operating under laws to protect the public from adverse health effects, limits the use of growth promotants; the EPA controls the use of certain pesticides and the result is less grain available after domestic use. The use of natural gas is limited in the production of fertilizers and farmers may have less fuel. All of this results in less grain at a higher cost. When less grain is available for export we have less money for low sulfur fuels and the air becomes less clean or control technology costs escalate. The point is that agricultural production and many other components of a highly technological society are very closely webbed with options for a clean environment. More attention should be placed on legislation consistent with integrated control and quality. It is the task of the toxicologist and statistician to provide the decision makers with data which can be used in establishing relative health effects.

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Summary Address for the Symposium— Statistics and the Environment

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As the pilot said to his passengers while trying to find his way along the coast in inclement weather, "Folks, I have some good news and some bad news", I can say that I have some good and some bad to report. But first the bad, then the good.

The objective of this Symposium as stated in the program was "to provide a forum for the interchange of ideas of mutual interest among experts in toxicology and environmental areas with specialists in the statistical techniques of data gathering and analysis. This is not a meeting where statisticians will speak statistically to their colleagues, or environmentalists will converse in their own language to their co-scientists. It is hoped that attempts to solve environmental problems will be enhanced by an interdisciplinary approach resulting from the communication among the pertinent professions."

If in fact this meant that we will solve many problems here, we have failed and failed miserably. If the purpose is, as

Dr. Sam Greenhouse stated yesterday, not to talk about threshold (this is peripheral but important) but rather put together our accumulated knowledge on how Dr. Henderson and others may continue in their research, then perhaps we have succeeded. If it is to determine how to establish the impact of concentrations on health, how to find ways to answer the question of what procedure to use for obtaining an assessment of impact of an increased pollution to the heart, the lung, limb malformations, then perhaps we have succeeded. No, we have not answered the question, but we have convinced ourselves that this can be anwered better by a team which involves both the environmentalist and the statistician. But let me say more about that later. First, I would like to answer the following questions:

1. What have we said here in general?
2. What have we said specifically—
   (a) the keynoters?
   (b) the environmentalists?
   (c) the statisticians?
   (d) the discussants?
3. What kind of response do I hear—
   (a) from individuals?
   (b) from associations?

Following this I will come back to the question—

4. What did we accomplish?

and finally—

5. Where do we go from here?

What Have We Said in General?

In general, the toxicologists, epidemiologists and other environmentalists presented some issues, some concerns, and a broad view of areas of environmental research. In some of these a great deal has been accomplished in the past, while in others we have hardly gotten off the ground. The reasons for the poor state of affairs in some areas varies all the way from lack of knowledge of what to measure, the inability to measure, the failure to determine—the population, the difficulty of transferral of one type of test on one population to a test on another population, etc. Low dose, safe dose, safe concentrations and thresholds seemed to pervade the discussions at regular intervals. The general view was that a great deal of reasearch *has* been performed but its applicability is at times questioned. So also, in some cases, is the statistical methodology employed to analyze these data. The statistician, on the other hand, may not come into these problems with 20 years of research experience in the subject matter area. He must develop some parallel expertise, not alone, but as a member of a research team including the environmentalist. There are too few statisticians who have worked for many years in epidemiology, toxicology, medicine, public health, etc.

Perhaps what I am asking for is an understanding and patience on the part of the environmentalist who wants cooperative assistance from the statistician. Just as he asks the statistician for patience in attempting to learn the problem, so also he must be patient. There are too many statisticians today who claim to be applied statisticians, whose concept of handling a real problem is to work with and manipulate the data in a vacuum, completely unrelated to the real problem. These people might be treating the number of students who obtained various grades on College Board Tests the same way as they would treat the number of deaths due to lung cancer from excessive exposure of one type or another. Statistical methods need not be wedded to one area alone. But statisticians cannot help solve real problems unless they know what the real data are and what these data mean.

What Have the Speakers Said?

Now let us be more specific about the presentations and discussions. What did they tell us (or me, a statistician)?

The Keynoters.—The keynoters did just that. Slightly out of sequence, let me turn first to the remarks of Mr.

Brownlee. From the point of view of our responsibility toward legislative action he said, "If I were to choose a theme (for us) to rally behind, it is that the scientific community must make itself more visible and available to policy makers than it has in the past. . . . The Congress has a pressing need for legitimate scientific advice, and it has been too hard to get it in the past." He pointed to the pathetic lack of technical expertise available to Congress, and the fact that Congressmen must make decisions on very little scientific knowledge and that we have failed to translate the fundamental facts to them in a simple layman's language. Just as we need an aggressive Congress so also we need aggressive scientists and aggressive scientific organizations to provide Congress with facts that they can understand. Essentially we have a choice—either to have legislation by an uninformed Congress or assure that Congress be informed and so hopefully make the proper decisions. Specifically I ask each of us "What have the organizations which we represent done to influence Congress by way of good scientific information?"

Dr. Vaun Newill in his keynote remarks asked each of us very pointedly: Who establishes research priorities, and how are these done? Who should support this research? How do we identify the problem and how do we assess the alternative solution? In short, we are faced with a certain environmental exposure which is a threat to our population or a segment of this population. The task then is to reduce the environmental risks and the resulting disbenefit. In an attempt to look for a solution we are faced with a host of common covariates. These must be considered if the solution is to be valid. He touched on the problem of balance of degree of health protection with the cost and, finally, the problem of implementation and enforcement.

Dr. George Box reviewed with us his concept of the iterative learning process, which goes from hypothesis via deduction to fact; then induction back to hypothesis, and the loop continues as a closed loop. To this the true state of nature introduces noise or random error. In all of this statistical method, the tool, the catalyst of the scientists is at the disposal of the scientist. But what of scientific method? While the closed loop depends on the wit and knowledge of the investigator, the statistician's job is to advise and assist in two crucial tasks. These relate (1) to the design and (2) to the analysis. The term design is used here quite loosely. In this, one must include not only experimental design where applicable but also surveys and the examination of past data. In essence, this includes the proper choice and collection of the data. Just as important is the analysis where the statistician plays a key role in interpretation of the data themselves, with the scientist drawing further conclusions from this. But in all of this the scientist and statistician together are faced with a world of variation and the problem of causation. To cope with this variation proper sampling techniques, weighting methods, test improvement, and economy of operations all play a role. In causation versus association, this problem cannot be solved by either of the two (environmentalist or statistician) alone, but together, if in fact it can be solved adequately in all cases. Finally, the research team in developing models must not err either in over simplicity nor over elaboration.

Carcinogens—Safe Doses?—In discussing the topic of safe doses of carcinogens, Dr. David Rall ruled out human epidemiological studies as not being of sufficient help to assure safety since (1) they take too long, (2) they are very expensive in available personnel and dollars, and (3) they are always subject to severe criticism. However, in animal studies there are such problems as pharmacological differences, receptor differences, temporal differences, and size differences. Once the translation from animal to human is attempted, we

are still faced with other differences, namely, those due to population—that is, its size, its genetic heterogeneity, its health, age, etc.—and those due to environment—that is, nutritional, chemical and physical.

Dr. Marvin Schneiderman next managed to place us in a world of trans-science, that is, that world of scientific problems which cannot be solved by the scientist himself. The statistician must live in this world and be a part of it and listen and plan and work with its other members. They must help determine just what problem is to be solved, what data are to be collected. Too often the wrong data are collected and analyzed about some other problem. Together the statistician and the environmentalist must design and plan and determine their domain and frame of reference. And together they must interpret. In the matter of the determination of safe doses, the statistician is faced with both non-statistical and statistical problems of estimation. Together they must determine risks, costs, benefits and many other criteria.

What can the biological scientist learn from the physical scientist? This topic was touched upon but with little depth. For example, the Weibull distribution was mentioned. This particular family of curves is used extensively by the industrial and mechanical engineers in reliability studies. It may prove most effective in the study of the effect of increase of doses. This particular curve has three parameters, or constants, which affect its location, shape, and range.

In the discussion following the earlier papers, Drs. Harold Peck and Jane Worcester emphasized the importance of stronger communications between the biologist and statistician, the cooperation of union and management in keeping and sharing good records. They discussed the problems of adequate sample size and proper confidence intervals. Even if, for example, the expected probability of an event is in fact 0.001, a sample size as large as 1000 tells us very little. If we have no occurrences of the event, the estimated probability is zero. But we have 95% confidence that it is at most 0.02. That is not too informative, is it?

Another topic which came up several times in our discussion sessions was that of accelerated tests. These are carried out rather successfully in engineering, though not without hazards in drawing conclusions. Though most discussants did not feel that there was a great applicability in biological studies, there is in my opinion a methodology well worth considering. Some tests certainly will not lend themselves to acceleration. But is this true in all cases?

Finally, the area of cost benefit analysis was explored briefly. Perhaps the greatest difficulty here is the continued attempt to consider this simply as a univariate problem when in essence it is specifically multivariate, both in input of control data and the response.

Air Pollutants—Safe Concentrations?—In the discussion of the topic of air pollutants Dr. John Finklea first posed a series of 12 questions about auto emissions and public health. Since this particular presentation was covered most capably by Dr. Morris Cranmer, I shall address myself to just two points—first on a personal tone, second as a statistician. He developed briefly the topic of "unrestrained advocacy". What a polite term for "you look out for your interest and I'll look out for mine." Perhaps this is too harsh. Yes, we are biased and we do have special interests. But let us, manufacturer or consumer, public or private, labor or management, toxicologist or statistician, not get into that Archie Bunker mentality of "Don't give me any facts, my mind is made up." Advocacy can be healthy, but there is some ground in between.

The other point concerns a task of rather monumental proportions. I wish I could take the time, the hours, the weeks necessary to explore with Dr. Finklea all of the twelve questions he

posed and to develop for each of these one or two simple, clear examples in his field. With these the students in my field could then obtain the motivation to work with him and his colleagues. Just as the biological scientist is encouraged in the utility of proper statistical analysis when he sees some statistical techniques at work (in his language), the statistician must see some clear, simple, short success stories. And each should be five pages or less if I am to present them to a student I want to interest in applied statistics. There are a number of things I would like to say further, but hopefully these will come out in the discussion.

Dr. John Hromi's presentation centered mainly around the concept of deterioration factor (D. F.). It is rather unfortunate that he did not develop further all of the statistical design and planning which went into this problem. There are many other facets of this problem which he touched upon such as the distribution of emission values. The underlying distribution in itself is key to the understanding and ultimate solution of many environmental problems. Add to this truncation of data and changes in the data base, and the problems are much more difficult. But what if a particular distribution cannot be assumed? What of the whole area of so-called distribution-free statistics? The biostatistician has done much with this. How much is the toxicologist using this?

The discussion which followed the two presentations on air pollutants was the highlight of the entire symposium. In this Drs. William Kirchhoff and Nozer Singpurwalla touched on such items as (1) the public breaking point, (2) the involvement of the statistician and mathematician in significant complex problems, (3) the multivariate responses, and (4) measurement errors.

There was some concern as to whether basic data have been generated in some areas, as well as the nature of these data. If the data do exist, who has them? Are there provisions for feedback? What is the implementation of planning and design? How good is the monitoring? Is the coordination among monitoring agencies adequate? Does it exist, or is there a serious fragmentation? In short, is there a concerted effort to evaluate and control the insult to the human body by all manners of pollutants?

Occupational Exposures—Thresholds?—Standards are legal implications. They alone do not accomplish the objectives. With this thought in the background, Dr. Richard Henderson proceeded to develop his topic of "Potential Hazards in Work Environment." He discussed comparative figures on urinary, fecal and biliary mercury concentrations. For each of these measurements we are still concerned with the distribution of the measurement and the effect of many other factors which create "noise". Are the data from the urine samples simply a set of random numbers? What about the problem of equal doses not being equal potential hazards? What does the threshold mean, and how is it related to some established standard? Should we recognize a distribution of standards? How can we use the distribution in estimation?

A report on "The Study of Steelworkers Mortality" was presented by Dr. William Lloyd. This study covered the years 1953–1961 and involved a population of 59,072 individuals. This is a most significant study in which expectations of deaths in various categories are compared with the actual results. Within this study of the Allegheny County steelworkers many factors were explored, such as causes of death, types of employment, location in the plant, white-non white employees and others. Even when drawing conclusions the issue of causation was treated carefully. Other studies of the same nature hopefully will be available to compare with this.

In the discussion which followed, Drs. Samuel Greenhouse and Charles

Powell explored the problems of the relationship of agent and human host, the purpose of the original data collection and their continued use at later dates, and the reluctance of a statistician to go on a "fishing expedition" for data when the random variable, the measurement and the parameters are not very clear at the early stages of the research.

What Kind of Responses Do We Hear?

From individual participants in this symposium on Statistics and the Environment we hear a range of responses. In part there is disappointment that we did not tackle the nitty-gritty of a number of real problems. We did a little, but perhaps too little. Could we have done more in this short time, with this format? From a very small minority we hear that all of this which we have discussed has been done. We are rediscovering the wheel? To these I answer humbly, "The wheel for one car may not fit another. Help us modify it and don't shout at us or you'll end up talking to yourself." From others, the majority, we hear a plea of cooperation, communication and team effort. To them I say: "Many of my statistical friends may not understand the toxicological problem. But don't confuse this with lack of interest and willingness. They may not call you. Perhaps it is because they are not aware of the challenge, the complexity, the full dimensions of the problem. Why don't you call them?"

But what do we hear from the organizations—those represented here and those of whom we are members? Organizations are slow to move. The older they are, sometimes, the longer it takes. Incidentally, the American Statistical Association was born in 1839. That simply means that there is more of a challenge to us. As associations we should begin to be aggressive. We represent professional societies. Why were these established? To write papers, to get brownie points or to be a service to the profession and to the small and large and larger community of which they are members? And when I say community I am not referring to inanimate structures, but people and families and brothers and sisters, however you define them.

In brief I believe that we have accomplished our purpose. No, we have not solved any problems, except the problem of learning about the word "communications". Let's keep talking and learning from each other and learning to work with each other.

Now, Where do we Go from Here?

I would be very sad if all of the activity of the past three days would end as we leave this National Academy of Sciences Auditorium. I somehow feel that there is a sort of personal mandate for each of us to move forward. Now what form shall it take? Let me ask about some possibilities.

1. Let me begin by informing you that I am exploring with Vaun Newill the potential of a closer relationship between ASA and EPA. Where it will go I don't know, but we shall explore.

2. Should we attempt to establish a more formal interrelationship between the statistician and the toxicologist on an Association level? Some of us will explore this.

3. Should we have a similar symposium in about 12 or 18 months with the same and expanded participants? This might start with preprints of problems to be presented and then explored in workshops.

4. Should we try to develop a Casebook—not lengthy, but with 10 to 12 case histories? What would be its purpose? And who would be the audience?

5. Should some permanent committee on an association-to-association level be established?

Those are some of the questions—now where do we go?

Summary Session Panel Discussion

Chairman: **Dr. Seymour L. Friess,** *Naval Medical Research Institute*

Panelists: **Mr. Leon G. Billings,** *Senate Subcommittee on Air and Water Pollution*
Dr. Morris F. Cranmer, *National Center for Toxicological Research*
Dr. Fred C. Leone, *American Statistical Association*

DR. FRIESS—That ends the formal program of the morning. I now open to the floor and to the panel a question-and-answer period in which you can range as broadly into the past and future as you like.

DR. MARVIN A. KASTENBAUM—I would like to start in the British tradition by thanking Fred Leone for his excellent summary of this meeting. I would like, also, to publicly offer my assistance to Fred and to the American Statistical Association in following up some of the proposals made at this meeting.

I was motivated to comment by a statement that Phil Kirchhoff made yesterday concerning the failure to study employees of a TNT factory in Chattanooga. He asserted that, having once failed to study this group, we have lost forever the opportunity to examine the effects of nitrogen dioxide on an exposed population. In response to this assertion I would remind you that all knowledge we have on the effects of radiation on the populations of Hiroshima and Nagasaki has been gathered retrospectively. Indeed the Atomic Energy Commission has spent over 25 years in an attempt to establish valid dosimetric measurements. Thus if nitrogen dioxide is sufficiently important in a population which is no longer being exposed, and if such a previously exposed population exists, it is certainly not too late to study that population exactly as the populations of Hiroshima and Nagasaki have been studied. These are not opportunities that we must necessarily consider as completely lost. We should rather address ourselves to the relative importance of specific questions that demand answers.

A similar suggestion applies to the question raised a few moments ago by Mr. Billings concerning the effects of disposal of solid waste in the soil. Many years ago the Atomic Energy Commission saw, as its obligation, the need to study the effect of disposal of all radioactive wastes on the soils and water systems in the areas where such problems arose. I'm not suggesting that this problem is exactly analogous to the one cited by Mr. Billings, but the fact is that this type of scientific input was available to Congress, and in that instance Congress called upon the scientific community to present available options. The responsibility for these matters is not entirely in the hands of the scientific community; some of it rests with Congress whose prerogative it is to call for this information when it needs it.

One point that I'm glad Fred brought up concerns the exchange of information among statisticians working in the areas of engineering, physical, and biological sciences. The subject of reliability and biometry is an excellent case in point. In this area engineers and actuaries have been working for many years on similar mathematical and statistical problems without a mutual exchange of information. What engineers have referred to as failure rates and hazard rates, the actuaries and biostatisticians have been calling death

rates, force of mortality, and mortality rates. The mathematical reduction of all of these concepts is identical. The statisticians in their respective fields of application have been talking about the same thing. Yet it wasn't until just recently that Frank Proschan finally brought this material together in a book called *Reliability in Biometry*. We must realize that such things as multiple risks, multiple component failures, and competing risks all deal with the same or similar problems. If the terminology is different, the statistical community must educate itself to this fact.

One point that Dr. Cranmer raised this morning concerns the serious question of decision-making. I believe that statisticians must address themselves to this question as it relates to the matter of professional ethics. Dr. Cranmer spoke of acceptable risks in terms of anti-cancer therapeutic agents. Anti-cancer therapeutic agents are generally used on human populations, and this practice has been interpreted by some people as human experimentation. We are told that the patients for whom these risky agents are prescribed may be more willing to accept the associated risk. The truth of this statement depends to a great degree on the alternatives that are presented to the patient. If the choice is between death and extension of life, then a relevant and important factor in this equation must be "the quality of life." It is not sufficient, therefore, for the statistician who examines the "end-results" of such studies to make decisions based only on the extension of life. He must also assign appropriate weights to "the quality of life."

This matter of decision making relates to the subject of teratogenesis raised by both Marvin Schneidermann and David Rall. Many years ago, in a major experiment in mouse genetics at the Oak Ridge National Laboratory, Dr. William Russell produced more mutations with 300 roentgens of radiation than with 1000 roentgens. On the basis of these results he might have concluded that 1000 r was less teratogenic than 300 r. Instead, he pursued his studies further until he found that 1000 r was killing a large number of immature sperm that hadn't had a chance to develop into mutants.

Finally, I will say a few words on the subject of trans-science. One of the examples that Dr. Alvin Weinberg cites in his MINERVA paper on the subject of trans-science was based on a real statistical problem presented to me in Oak Ridge. It was the usual question posed to statisticians concerning the size of sample necessary to detect a prescribed difference in the means of two samples. The spontaneous mutation rate from Dr. Russell's strain of mice is 56 in a million. This means that during the past quarter of a century, Dr. Russell has looked at almost a million mice and has counted almost 56 mutations. It is not a number to be treated lightly. The question that was asked is: How large a sample would be needed to demonstrate, with a certain degree of alpha and beta risk, that a small dose of radiation increases the mutation rate by 0.5%? The answer to this question, using standard statistical techniques, is 8 billion mice. This doesn't mean that the experiment can't be done; it simply means that nobody in his right mind would do it. And this is trans-science.

MR. BILLINGS—I'm rather interested in the comment concerning the AEC studies on disposition of solid-liquid radioactive materials, and the extent to which the knowledge and information developed by those studies is transferable to polyethylene bags and aerosol cans.

DR. KASTENBAUM—I said that they were not directly analogous. In order to get this information, Congress needs to ask its questions seriously. It should not expect that answers will be volunteered for questions that have not been asked.

DR. FRIESS—I'd like to comment on current modes of the interaction between Congressional committees and

scientists, particularly in that there appears to be a breed of scientist who is a born testifier, with influence directly depending on his fluency with words.

MR. BILLINGS—We call those scientific streakers today.

DR. FRIESS—Is there any way that Congressional committees can select the kind of scientific talent they want for consultation, or are they at the mercy of whoever happens to step forward for one motive or another?

MR. BILLINGS—Well, we tried something a little bit unique when we wrote the 1972 Water Act. We assigned a scientist to our staff—that is unique for [the] Congress, I can assure you. He did some really superb things in helping us put some flesh on definitions of toxic and hazardous materials and son on. He was able to reach into the scientific community. I'm absolutely sure he reached a number of people we couldn't have, but I'm not sure that the people he reached were any better or worse than the people who would reach out to us on a regular basis. My eight years of experience on this subcommittee has been that we try to absorb all the information we can get. The scientific loudmouths are the best things going because they're the only ones we can hear. We have a limited capacity to understand the very technical published material, so we have to depend on the scientists who can speak in plain language. That tends to cause oversimplification and a certain amount of scientific horror among their colleagues. On the other hand it is the only really relevant information that we get. Communication must be initiated by the scientific community, because we don't know who they are, and we have no way of asking the appropriate persons for it. We need people to come forward who can express fairly complex information in layman's terms. We know some of the words, most of us can read, and some of us even listen. Let me give you an example of the problem we have. Up until two years ago the staff of the subcommittee on Air and Water Pollution consisted of one person—myself. We had one excellent minority staff person who had scientific credentials—this was relatively rare. That was the total subcommittee staff capacity directed toward the writing of the Clean Air Act. Well, that gives you absolutely no time to absorb other data, especially when you are writing speeches, articles, and reports, conducting hearings, and so on. I think I have put some of the parameters on our problem.

DR. LEONE—I know that each committee decides its own method of obtaining witnesses. Does yours ask the associations to recommend a key witness in this area or that? Once you go to the top level of an association, there is no axe to grind, no prima donna to take care of, no loudmouth to worry about. In fact, they would avoid these people because they would recommend their most competent individual. In fact, in such a thing as the Clean Air Act, perhaps an association can identify people who would spend time working on it as part of their professional activity.

MR. BILLINGS—Let me make a couple of comments. I doubt that I can tell you the names of three or four associations, or how many there are. That is how poor the communication has been. It's partly my fault—mostly limitation of time. Our experience with most associations outside the scientific community has been that the association has represented the lowest common denominator of any point of view, rather than a generally useful perspective from the best elevation. By the way, when I use that word—loudmouth—I use it kindly. If it hadn't been for loudmouths there wouldn't have been any push. We have gone to what academic institutions we could, and obtained information from what we have been able to read. When anyone has something to say in the science magazines, we call him up and

ask him to come down and testify. I think my brother is a member of the American Statistical Association, but other than that, I had never heard of it before. And I never heard that there was a toxicological association, either. I know there are toxicologists. We depend on EPA labs and EPA people like Jack Finklea and David Rall at NIEHS. We didn't even know who they were until they came out of the woodwork when the Nixon administration decided to kill science. There are some real limitations. I don't feel embarrassed about it all because I know why we haven't been able to find you, but I wonder why you haven't been able to find us? I suppose that is part of the problem.

MR. WANDS—I don't want to pick on our fresh guest too heavily this morning, but you'll recognize him as a newcomer to our entire group. We've been at each other's throats for the last two days, and you've just arrived, Mr. Billings, so we will direct quite a few of our questions to you this morning. I'm glad to hear that there is a mutual seeking of each other's help. The mutual frustration is not knowing whom you're reaching for. I can tell you that the scientific community by and large—particularly toxicology and other environmental types of sciences and of course, as Dr. Leone has said, the statistician community—is most anxious to be helpful at any stage in the legislative process. We can offer you reliable and responsible witnesses who will have credibility within the scientific community and who will have communicability to the lay public.

Now I would like to ask you two questions, if I may. One, the Nation's largest employer is the U. S. Government. How does it stand in relationship to environmental laws which Congress has passed? What is its status as to compliance with those regulations? Two, during the last two days, we have skirted around a basic issue which was discussed here at the Academy a few months ago—how safe is safe? The question has been the concept of the acceptance of some degree of risk——maybe a very infinitesimal risk such as one case of cancer in a hundred million. That number has been published and bandied about here. Perhaps you have a little better finger on the public pulse regarding this kind of situation than we in the scientific community have. How do you feel about the possibility of assessing the public's willingness to accept some degree of risk in order to maintain the life style of which you spoke?

MR. BILLINGS—You asked two questions and I'm giving three answers. First, on your question of being helpful, remember that limited capacity we have to seek you out. When we have a problem we will appreciate your volunteering the best assistance that you have. I really would be making false promises if I suggested any real possibility of us knowing well enough what kind of input you and your colleagues would be able to make sufficiently in advance to go out and get it.

Second, the Federal Government's job in environmental improvement is relatively adequate but not if you assume that the national government was expected to take a leadership role in these areas. Let me give you one example. The Tennessee Valley Authority is one of the leading utilities undermining the Clean Air Act and not responding to its challenge. I must say that the Defense Department has been remarkably effective in most areas, partly because their funding is more adequate.

Now as to "how safe is safe?", which I think is the most important question you asked. There is a tendency to put policy makers into the position of having to define some acceptable degree of risk based on scientific evidence, or conversely to show that which occurs is not unacceptably risky. Now I don't really know how you cope with that problem except in these ways. We are living in a technological society—

technology is responsible for the life style which both you and I prefer. It seems elemental to insist that the minimum controls on environmental degradation be those which are technologically possible. After you have achieved that which can be done, you have an opportunity to evaluate what remains to be done. The fact of the matter is that, short of draconian measures, you are not going to effect many enviromental improvements beyond the limits of technology. The most difficult changes to make are beyond the limits of technology. You then get into the alteration of life styles, transportation and consumption patterns and so on. Those are the last changes you want to make, because it is then that you really get into the public's defining that degree of risk. Once you have said, "All right, the best you can get out of the technological pig is this particular squeal," then the public can say, "Well, we are willing to take the risk because we don't want to make these changes." Then policy makers get into the extremely difficult and often untenable political position in which a majority of the public least exposed to the risk is willing to impose a greater risk than the minority of the public is willing to accept—the infirm, the old, and the young. But now there will be a problem in the air quality area in places like the Los Angeles basin, where the people won't want to change their living pattern and where the limits of technology will be achieved. There will still be unacceptable levels of risk to health for a considerable sector of the people. I doubt that there is an easy or even an acceptable political response to this problem, but on the other hand I have some faith that the response will come about. It will take a lot more time than we envision in the Clean Air Act.

DR. CRANMER—I would like to make a few comments, if I may, on a point that we often bandy about in discussions at our laboratory. We must accept the fact that you can never prove something absolutely safe. It is not playing with words when one says there is a relativeness to all safety evaluation. Often the relativeness is ignorance. I wish to also comment on statements made about the input from the AEC program. The AEC has much information which can and has been used in drawing comparisons to chemical toxicology. When I described Druckrey's model, I should have introduced my statements by describing Blum's ultraviolet work which certainly fit the model and was developed earlier.

Dr. William Russel of the Oak Ridge National Laboratory, Division of Biology, has developed a program comparing the relative mutagenicity of chemicals with a broad background of information that he has accumulated with radiation. It may indeed, as mentioned, take eight or ten billion mice to adequately describe a mutation rate of 56 in a million with a half a percent increase in that rate. Let us not forget, however, about the materials which produce this absolute mutation rate of a percent or so in experimental animals. This potential describes a completely different aspect of the problem. Where are we in chemical mutagenesis? In mammals, we are only able to detect the very potent mutagenic agents. If the compounds of interest are weak or have a high acute toxicity, or produce recessive mutations, we have hardly any tools at all. We hope we will be able to attack the problem by utilizing some of the genetic trace capabilities of large animal colonies at NCTR. For instance, in my presentation I said we were concerned that spontaneous mutation might occur, and we developed a system to trace and identify. We can also administer mutagenic compounds to animals and identify mutations that occur by the same multiple generation trace capability. The same type of logic base is required. There are also other carryovers from the radiation program. In some cases, higher doses of radiation produces fewer cancers than slightly lower doses. The cancer data at the higher doses suggest the radiation had a

protective effect and indeed it might have killed some cells which had, or would have, been transformed. Studies at NCTR with 2-AAF chemical carcinogens show a lower observable incidence of tumors at high doses. We partially explain this by observing that if you kill the animal, it isn't available to develop a tumor. However, there remains an unexplained component which may be parallel with radiation effects, cell death rather than transformation.

In summary, I don't think we have done as badly as might appear in terms of utilizing information that's been accumulating through AEC programs. We are actively bridging the gap between these two activities.

DR. DOUGLAS BALLARD—There is a very good paper on the risk due to coke ovens. My first comment is that if you ask any steel worker, he could have told you there was a risk 50 years ago. Relative to the risk in coke ovens, the people who will run coal gasification plants to solve the energy crisis are going to face the same risks. A minority of people will have to face the risk of converting coal to gas to run our cars. How are we going to trade this off?

MR. BILLINGS—One of the answers is that a way to avoid that risk entirely is to do something with the automobile itself.

DR. ISRAEL ROTKIN—Instead of considering only the risks and benefits inherent in any particular activity, it would be more useful to consider together the risks and benefits of all alternative ways of achieving the same human goal. Apparently, the easiest way to avoid 50,000 deaths per year in the USA would be to abolish all automobiles here or, as someone suggested to me yesterday, to slow them all down to 5 mph. There is no doubt that it would be effective in the sense that almost no one would be killed by an auto. However, our death toll would not be reduced by 50,000. Alternative means of transportation would be needed. People were killed by horses too. A scientific audience like this should recall that the co-discoverer of radium, Pierre Curie, was killed by a horse-drawn truck. Not only were people run down by horses, but they were also killed by diseases transmitted by horses. Thus, to save lives, it is not enough to consider the lethality of the auto; the lethality of all reasonable alternatives must also be taken into account. In general, in spite of the difficulty, we must find out which way of behaving would result in the greatest overall benefit to mankind. This approach would be much more useful than the concept of risk as it has been treated here.

I must add that I don't like the idea of turning exclusively to authoritative organizations. I remember Louis Pasteur; had the world listened to the authoritative organizations at the time he was making his proposals, we might be dying of infectious disease to a much greater extent.

DR. FRIESS—Thank you. We are approaching the end of the morning. I should like to take the chairman's prerogative on behalf of all the participants, all the speakers, all the panel members, and the audience to thank our host for the use of this wonderful auditorium, and the chance to explore the field as we did. I would also like to assure those of you who are still with us that the Executive Committee will carry on from this point to determine the future progress and planning of follow-up symposia, if they are so indicated.

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			Approx No. of Minutes
Keynote Session			
____	# 1	RALPH C. WANDS: Introduction to the Symposium	20
____	# 2	VAUN A. NEWILL: Regulatory Decision Making: The Scientist's Role	45
____	# 3	MICHAEL BROWNLEE: Keynote Address: Statistics and the Environment	45
____	# 4	GEORGE E.P. BOX: Statistics and the Environment	45
____	# 5	KEYNOTE SESSION DISCUSSION	45
Carcinogens—Safe Doses?			
____	# 6	BEATRICE S. ORLEANS: Opening Remarks	10
____	# 7	NANCY R. MANN: Introduction	10
____	# 8	DAVID P. RALL: Problems of Low Doses of Carcinogens	45
____	# 9	MARVIN A. SCHNEIDERMAN: Safe Dose? Problem of the Statistician in the World of Trans-Science	45
____	# 10	PANEL DISCUSSION	60
Air Pollutants—Safe Concentrations?			
____	# 11	HENRY LATHROP: Introduction	10
____	# 12	JOHN F. FINKLEA: Auto Emissions and Public Health: Questions, Statistical Problems, and Case Studies	45
____	# 13	JOHN D. HROMI: Some Aspects of Determining New Motor Vehicle Engine Emission Levels	45
____	# 14	PANEL DISCUSSION	90
Occupational Exposures—Thresholds?			
____	# 15	BERTRAM D. DINMAN: Introduction	10
____	# 16	RICHARD HENDERSON: Thresholds for Control of Potential Hazards in Occupational Environments	45
____	# 17	J. WILLIAM LLOYD: Study of Long-Latent Disease in Industrial Populations	45
____	# 18	PANEL DISCUSSION	60
Summary Session			
____	# 19	SEYMOUR L. FRIESS: Introduction	10
____	# 20	MORRIS F. CRANMER: Reflections in Toxicology	45
____	# 21	FRED C. LEONE: Summary Address for the Symposium	45
____	# 22	PANEL DISCUSSION	45

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CONTENTS (Continued from Front Cover)

VOLUME 64
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CONTENTS

Features

Centennial of Gibbs' Thermodynamics—A Symposium

Profile

Research Reports

(Continued on Back Cover)

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FEATURES

CENTENNIAL OF GIBBS' THERMODYNAMICS

The paper appearing below and the four that follow were delivered as a symposium at the 543rd meeting of the Washington Academy of Sciences in the John Wesley Powell Auditorium of the Cosmos Club, Washington, D.C. on Feb. 21, 1974.

Centennial of Gibbs' Thermodynamics— Introductory Remarks[1]

Raymond J. Seeger

National Science Foundation (Ret.)

When the Göttingen Physical Chemist, Walther Hermann Nernst (Nobel Prize in Chemistry, 1920) gave the Silliman Lectures at Yale University in 1906, he inquired if there were any memorials to Gibbs. "The chemist, Oliver Wolcott Gibbs?" was the query. "No, the physicist, J. Willard Gibbs" was the reply. Accordingly, Nernst gave Yale his honorarium ($500) as an initial donation for a memorial to the outstanding American Scientist of the nineteenth century, possibly the outstanding American theoretician ever. At that time Gibbs was appreciated more in Europe than in the United States—probably still true today. In 1912 a bronze portrait tablet was placed in the then new Sloane Physics Laboratory at Yale; in 1955 it was moved to the new J. Willard Gibbs Research Laboratory.

Gibbs has always been identified with New Haven where he was born February 11, 1839; his father was a theological scholar at Yale College. At the age of 10 he began attending the colonial Hopkins Grammar School. At fifteen he entered Yale College where he received the classically oriented B. A. and a Phi Beta Kappa Key. At 19, he commenced graduate work in engineering and later earned the first Ph.D. degree in engineering in the United States. At 24, he accepted a 3-year appointment as a Tutor in Yale College. The first two years he was required to teach Latin; the third year he was free to select his own interest, namely, natural philosophy. At 27, he left his habitat with his two sisters for three years of informal postgraduate study at the European universities of Paris, Heidelberg, and Berlin. At the time of the departure abroad one could rephrase the autobiographical comment of Henry Brooks Adams at the conclusion of his own Harvard schooling, namely, "the scientific education of J. Willard Gibbs had not really begun."

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[1]R. J. Seeger, "J. Willard Gibbs, American Physicist par Excellence," Pergamon Press (1974).

After his return, at the age of 32, Gibbs was appointed Professor of Mathematical Physics in Yale College. At 34 (1873), he published two papers on "Graphical Thermodynamics" in the Transactions of the Connecticut Academy of Arts and Sciences. A third paper, "Heterogeneous Equilibria," was published in two parts in 1875 and 1878. At 63, Gibbs published a book written specially for the Yale Bicentennial (1901). It was a unique contribution to classical physical science in that it related continuum thermodynamics to kinetic theory; despite the subsequent development of quantum theory it still remains the statistical mechanics foundation for so-called rational thermodynamics.

On April 28, 1902, Gibbs died. He was buried in Grove St. Cemetery near his home and office.

Though Gibbs was elected in 1879 to the National Academy of Sciences, he was never a member of the American Physical Society or of the American Chemical Society, and he joined the American Mathematical Society only shortly before his death. On April 1, 1900, however, Charles Walcott, President of the Washington Academy of Sciences, wrote to H. E. Hadley at Yale to urge him to encourage acceptance by eight Yale faculty recently elected to the Washington society. Gibbs accepted; he was a member of the Washington Academy of Science, hence this occasion to commemorate the centennial of his epoch-making thermodynamics.

A Geometrical Description of Critical Phenomena

Raymond D. Mountain

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ABSTRACT

Gibbs made extensive use of geometrical concepts in his development of thermodynamics. In this talk we examine the use of geometrical ideas to clarify our understanding of the thermodynamics of fluids in the vicinity of the critical point. The influence of Gibbs on recent developments in the study of critical phenomena is emphasized.

The study of thermodynamics described in Gibbs' three papers is a rich source of information and insight for the contemporary student.[1] Today I want to focus our attention on Gibbs' use of the geometrical features of thermodynamics in his development of the mathematical structure of the theory. He began with the formulation embodied in eqs. (1) and (2) (*vide infra*) and went on to develop thermodynamics to the point where it is not conceptually distinguishable from presentations found in current texts. After we have examined Gibbs' use of geometrical relationships in formulating thermodynamics, we shall see how the use of geometry has aided our understanding of the unusual thermodynamic properties observed in the vicinity of the liquid-vapor critical point.

Let us begin by briefly considering the content of Gibbs' three papers. The first

[1]Gibbs' work on thermodynamics is reprinted in *The Scientific Papers of J. Willard Gibbs, Vol. 1*, Thermodynamics. Dover Publications Inc., New York (1961). This, in turn, is a reprint of the volume originally published by Longmans, Green and Co. in 1906, 3 years after Gibbs died.

paper, published in 1873, considers various 2-dimensional representations of thermodynamic properties. P-V diagrams were in common use at that time, and he explored alternative ways of representing thermodynamics. In addition to showing that T-S diagrams have useful properties, he also investigated other possibile combinations of thermodynamic variables and some of the properties of such diagrams.

Later in 1873 he published a second paper on the use of surfaces to represent thermodynamics. By that time the experiments of Andrews on critical phenomena in CO_2 had stimulated Thompson to suggest that a PVT surface would be a useful way of representing thermal properties. Gibbs in a footnote (as frequently occurs in his work) takes note of Thompson's work and points out that the surfaces Gibbs is considering have a much greater information content than do Thompson's. The sort of surface Thompson has in mind is shown in Fig. 1. This is the familiar PVT surface which represents observations of properties faithfully but does not convey the essence of thermodynamics. Gibbs was proposing instead to use a surface representing the entropy, energy and volume of the fluid from which the equation of state can be obtained by differentiation. That is, the energy U is a function of the entropy S and the volume V subject to the condition

$$dU = TdS - PdV \tag{1}$$

He then proceeded using eq. (1) and the by then known condition for thermodynamic stability

$$(\delta S)_U \leq 0 \tag{2}$$

to deduce the convexity properties of the U (S,V) surface. Again in a footnote the conditions for the equilibrium among different parts of the system are stated. These conditions are that the temperature, pressure, and what we now know as the chemical potential be uniform throughout the system.

In his third and major paper on thermodynamics, 1876–1878, the subject is explored and set out in a very elegant and complete way. The ideas in the footnotes in his second paper are (among many others) expanded and applied to a variety of interesting situations. For the purpose of the discussion at hand I want to focus on the quantity now known as the Gibbs free energy and the property that the chemical potentials (Gibbs free energy per unit mass) for phases in thermodynamic equilibrium are equal. The type of surface that results is shown in Fig. 2.

The advertised topic of this talk is critical phenomena. For one-component

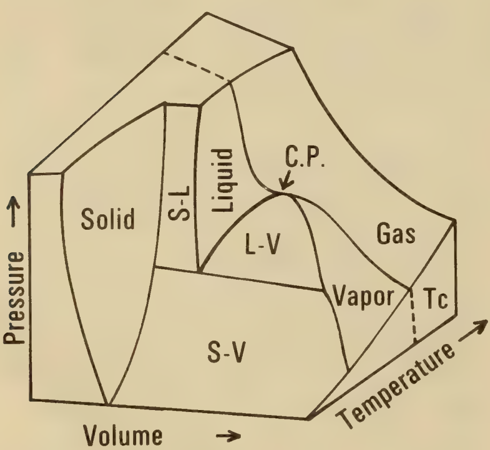

Fig. 1. A pressure-volume-temperature surface for a substance which contracts on freezing. The critical point (C.P.) and the critical isotherm (T_c) are indicated.

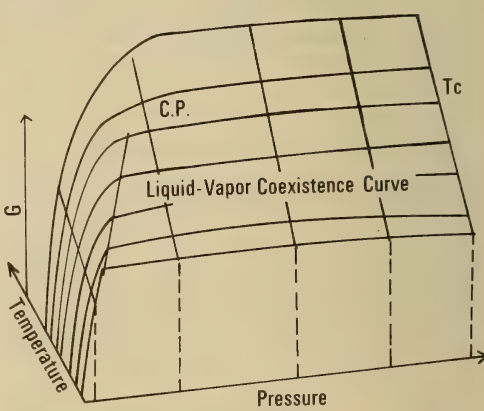

Fig. 2. A portion of a Gibbs free energy (G) pressure (P) temperature (T) surface in the vicinity of the critical point (C.P.). The critical isotherm (T_c) and the liquid-vapor coexistence curve are noted.

fluids the critical point is the point where distinction between liquid and gas vanishes. It is indicated in Fig. 1 at the top of the liquid-gas dome. Gibbs realized that the critical point is characterized by a horizontal isotherm with 0 curvature. It is displayed in Fig. 1 as the critical isotherm T_c. The critical point can be characterized by the conditions

$$\left(\frac{\partial P}{\partial V}\right)_T = 0, \quad \left(\frac{\partial^2 P}{\partial V^2}\right)_T = 0 \quad (3)$$

Critical phenomena is a topic which has, over the years, come and gone as a popular subject for scientific research. It has grown in interest because of its obvious significance for the study of thermodynamic properties of matter and because of its own intrinsic scientific interest. The study of critical phenomena has declined in time because of the lack of adequate experiments (which are quite difficult) and the corresponding difficulty of developing useful theory. During the 1950's and early 1960's there was a renewed interest in the study of critical phenomena. This time the interest in the subject did not die out but has grown into a full-fledged, mature understanding of critical phenomena. A turning point can now be recognized as occurring in 1965 when a conference on critical phenomena was held at the National Bureau of Standards.[2]

Shortly after that conference a number of things happened which have a definite Gibbsian flavor to them. One of these was the publication of a paper by Widom[3] which analyzed the thermodynamics of the critical region in ways that had not been used before and which were consistent with the anomalous properties of thermodynamic quantities. Built into this work is the crucial idea that in some way critical phenomena are universal features of matter, independent of the specific fluid in question.

Now what are the significant features of thermodynamic properties in the vicinity of the critical point? They can be characterized by divergences in the isothermal compressibility

$$K_T = -\frac{1}{V}\left(\frac{\partial V}{\partial P}\right)_T \sim (T - T_c)^{-\gamma}, \quad (4)$$

$$\gamma \simeq 1.2$$

in the specific heat at constant pressure

$$C_P = \left(\frac{\partial U}{\partial T}\right)_P \sim (T - T_c)^{-\gamma} \quad (5)$$

and in the specific heat at constant volume

$$C_V = \left(\frac{\partial U}{\partial T}\right)_V \sim (T - T_c)^{-\alpha}, \quad (6)$$

$$\alpha \sim 0.1$$

The topic we wish to pursue is the understanding of these anomalies from an essentially geometrical point of view. The Gibbs free energy-pressure-temperature surface, Fig. 2, can be characterized mathematically as

$$\begin{gathered} dG = -SdT + VdP, \\ (\delta G)_{P,T} \geq 0 \end{gathered} \quad (7)$$

The only striking feature of this surface is the ridge representing the liquid-vapor coexistence curve. The approach to the critical point is characterized by having this ridge become less and less pronounced and finally vanishing at the critical point.

This can be envisioned by taking a sheet of paper and cutting a pie-shaped wedge out of it as indicated in Fig. 3. Take the sheet of paper and fold the cut-off wedge so that the edges touch. Then note at the point of the wedge that the curvature is different depending on whether one goes along the line which is the extension of the cut or whether one goes at some other angle. The strong and weak divergences observed in the compressibility and in the heat capacities reflect the different curvatures in the Gibbs

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[2] *Critical Phenomena*, Proceedings of a Conference; Editors: M. S. Green and J. V. Sengers. U. S. Government Printing Office, Washington, D. C. 20234, (1966). NBS Misc. Publication 273.

[3] B. Widom, J. Chem. Phys. **43**, 3898 (1965).

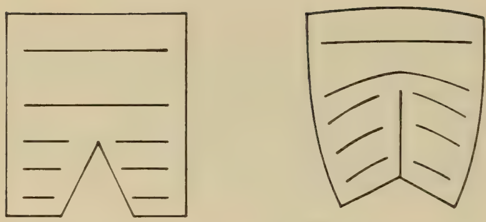

Fig. 3. A rough model of the Gibbs free energy surface can be made by cutting and folding a sheet of paper as indicated. The straight lines on the flat sheet of paper become curved when the sheet is folded.

free energy surface in the vicinity of the critical point, and *that is the essence of the geometrical characterization of the anomalies occurring in the critical region.* This concept was set forth in a thermodynamic theory of critical phenomena by Griffiths and Wheeler in a 1970 paper.[4] The theory, which is geometric in flavor, makes use of the properties of the Gibbs free energy surface in a definitely Gibbsian way to elucidate the thermodynamics of the critical region and to predict how multicomponent critical phenomena will (and apparently do) manifest themselves.

There are two essential features of this theory. The first is the idea of universality; that is that the shape of the Gibbs free energy surface near the critical point is independent of the substance. The second feature is that there is only one "direction" of any significance, that of the coexistence curve. The curvature along an extension of the coexistence curve characterizes the weak divergence observed in the specific heat at constant volume, while the curvatures associated with other directions characterize the strong divergence observed in the isothermal compressibility and the specific heat at constant pressure. These ideas lend themselves to a geometric view of multi-component critical phenomena with predictions which appear to be borne out with experiment, the ultimate test of any theory.

In summary, Gibbs was able to develop thermodynamics from a set of tentative empirical statements into a full-fledged, mathematically sophisticated physical theory through the insights gained through the geometrical considerations which he employed so skillfully. In recent times this geometrical viewpoint, when applied to the problem of the properties of matter near critical points, has been invaluable in sorting out and clarifying the many observations of anomalous properties. This is an outcome which would not have surprised Gibbs in the least, and I am sure it would have pleased him.

[4]R. B. Griffiths and J. C. Wheeler, Phys. Rev. A, 2, 1047 (1970).

Gibbs Phase Rule and Its Application in Geochemistry

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ABSTRACT

In his treatise "On the Equilibrium of Heterogeneous Substances" Gibbs developed a method of formulating multicomponent, multiphase equilibria that remains useful today. Gibbs wrote down a system of linear differential equations consisting of one of his equations (97) (the "Gibbs-Duhem" equation) for each phase together with such conditions of equilibrium as are necessary to eliminate dependent components. To this system of equations may be added others which give the relationships between pressure, temperature, exchange potential, and phase composition. A complete discription of a heterogeneous system in chemical equilibrium can be so obtained in terms of the variables pressure, temperature, chemical potentials of components, and phase composition. This review paper shows how Gibbs' method can be applied to rocks in order to extract from them information on the conditions under which they formed.

"A system of r coexistent phases, each of which has the same n independently variable components is capable of $n + 2 - r$ variations of phase. . . . Or, when the r bodies considered have not the same independently variable components, if we still denote by n the number of independently variable components of the r bodies taken as a whole, the number of independent variations of phase of which the system is capable will still be $n + 2 - r$."

—Gibbs, 1876, reprinted 1961 by
Dover Publications, pp. 96–97

The phase rule has its chief geochemical application in the field of petrology, the study of the origin of rocks and the minerals that compose them. Petrologists are faced with the problem of locating and mapping rocks at or near the earth's surface and deducing under what conditions of pressure (P) and temperature (T) they were formed. Many studies show that the chemical and textural characteristics of rocks originating at great depth are preserved at the earth's surface. Thus information on the ambient conditions deep beneath the earth's surface is contained in rocks accessible to anyone owning a rock hammer. The petrologist uses estimates of P-T conditions for specific assemblages of minerals in rocks together with their spatial distribution mapped at the earth's surface to reconstruct the thermal structure of the earth at various times in the geological past. Such studies present one of the few methods available for testing geophysical models of the earth's evolution. Gibbs phase rule is indispensible in this work in at least two respects: first, it is used to interpret laboratory experiments designed to reproduce rocks and minerals under controlled P-T conditions; second, it is used to learn whether the variance of naturally occurring mineral assemblages is comparable with that of systems investigated experimentally and, therefore, whether the laboratory results are applicable to estimating the P-T conditions of natural mineral assemblages.

The purpose of this review is to show how the analytical methods used by Gibbs in deriving the phase rule can be applied to rocks in order to obtain information on the conditions under which they originated.

Problems in Application of the Phase Rule to Rocks

The phase rule cannot be indiscriminately applied to rocks and minerals.

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[1] Gibbs' equation (97) (*op. cit.*, p. 88) is so named in honor of its independent derivation by Gibbs and by Pierre Maurice Marie Duhem (1886, p. 33).

Various potential difficulties present themselves and have been thoroughly discussed (Thompson, 1959, 1970; Zen, 1963; Weill and Fyfe, 1964, 1967; Korzhinskii, 1959, 1966, 1967). Three of these difficulties are worth emphasizing, as they limit the applicability of the phase rule and the validity of conclusions drawn from it.

1. A given rock may have been subjected to a variety of geological processes prior to the time it is collected at the earth's surface. Its mineralogical characteristics may record conditions at a specific depth in the earth or they may record the conditions of a variety of environments that were experienced in succession. In order to obtain meaningful information on conditions beneath the earth's surface it is essential to apply the phase rule only to assemblages of coeval minerals. By taking into account the geological history of an area together with the successive mineral assemblages found in its rocks, it is usually possible to construct a chronology of mineral crystallization. In view of the many geological processes acting to alter ancient or deep-seated rocks it may seem surprising that any evidence can survive. Nevertheless, measurements of radioactive isotopes and their daughter products indicate that the mineralogical record of events in the earth's crust as old as 3.5 billion years has been preserved. Similarly, the occurrence at the earth's surface of high-pressure minerals such as diamond proves that rocks can be brought from depths of 150 km or more without alteration.

2. The phase rule is valid only for systems in equilibrium. Before applying the phase rule it must be determined that a given assemblage of coeval minerals was in equilibrium at some time in its geological history. It canot be verified by direct measurement that the equilibrium conditions of uniform P, T, and chemical potentials of components (μ_i) (Gibbs, *op. cit.*, p. 65) were met at some time in the past history of the rock. One of the most powerful indirect methods for judging whether a rock was once in equilibrium is to measure the chemical compositions of its coeval minerals to learn whether they obey the rules of equilibrium phase diagrams as set forth by Gibbs in his chapter on "Geometrical Illustrations" (*op. cit.*, pp. 115–129; cf. Kretz, 1959; Zen, 1963; Greenwood, 1967).

3. Components of each mineral (or phase) must be chosen so that they satisfy the conditions laid down by Gibbs: "The substances S_1, S_2 . . . S_n of which we consider the mass composed, must of course be such that the values of the differentials dm_1, dm_2, . . . dm_n shall be independent, and shall express every variation in the composition of the homogeneous mass considered, including those produced by the absorption of substances different from any initially present. It may therefore be necessary to have terms in the equation relating to component substances which do not initially occur in the homogeneous mass considered, provided, of course, that these substances, or their components, are to be found in some part of the whole given mass" (Gibbs, *op. cit.*, p. 63; cf. Thompson, 1959, pp. 445–448). If the components of each mineral in a rock are so chosen, then all the components of all the minerals of the rock (or system) will not necessarily be independent. Gibbs provides the equations necessary to eliminate the dependent components when systems of more than one phase are considered (Gibbs, *op. cit.*, p. 72, eq. 43; p. 74, eq. 51). The practical consequence of Gibbs' definition of the components of a phase (or mineral) is that the study of a single rock specimen is not likely to afford a complete list of all the components of its minerals. The enumeration of the components of a given mineral will change as more data are collected about the range of its possible compositional variations and as more sensitive techniques are used to measure its composition.

Gibbs' Analytical Formulation of Phase Equilibria

As stated in the quotation at the head

of this paper, the Gibbs phase rule relates the number of phases and components in a system to the number of independent variations of which it is capable, and this is the form in which it is customarily presented. Gibbs (*op. cit.*, p. 88, eq. 97) derived the phase rule by considering a set of simultaneous equations consisting of the "Gibbs-Duhem"[1] equations together with such equations as are needed to eliminate dependent components. As Morey (1936, p. 233) once observed "the phase rule itself is but an incidental qualitative deduction from these equations." Morey's opinion suggests that applications of the phase rule might profitably follow the analytical treatment pioneered by Gibbs (*op. cit.*, pp. 97–100) and so ably elaborated by Morey (Morey and Williamson, 1918; Morey, 1936) rather than concentrating on the numerical relations between variance, components, and phases. If the analytical method is followed rigorously, of course, the relations between variance, components, and phases will be implicit in the systems of simultaneous equations. The remainder of this review will describe how to obtain information on the environment in which rocks form by the analytical formulation of their mineral equilibria.

Univariant Equilibria

Consider a naturally occurring example of a univariant equilibrium in a ternary chemical system. There are four minerals (A, B, C, D) and three components (1, 2, 3). The four minerals are all ternary solid solutions such that the same three components can be chosen for each mineral. For each mineral a Gibbs-Duhem equation (Gibbs, *op. cit.*, p. 88, eq. 97) may be written:

$$0 = S_A dT - V_A dP + m_{1A} d\mu_1 + m_{2A} d\mu_2 + m_{3A} d\mu_3 \quad (1)$$
$$0 = S_B dT - V_B dP + m_{1B} d\mu_1 + m_{2B} d\mu_2 + m_{3B} d\mu_3 \quad (2)$$
$$0 = S_C dT - V_C dP + m_{1C} d\mu_1 + m_{2C} d\mu_2 + m_{3C} d\mu_3 \quad (3)$$
$$0 = S_D dT - V_D dP + m_{1D} d\mu_1 + m_{2D} d\mu_2 + m_{3D} d\mu_3 \quad (4)$$

where S_A and V_A are the entropy and volume, respectively, of phase A, and m_{1A} is the number of moles of component 1 in phase A. The chemical potentials μ_1, μ_2, and μ_3 were defined by Gibbs (*op. cit.*, p. 63) as "the differential coefficients of (the internal energy) taken with respect to m_1, m_2, and m_3," e.g., $\mu_1 = (\partial E_A/\partial m_{1A})$ at constant S_A, V_A, m_{2A}, and m_{3A}.

It is more convenient to use these equations in a slightly different form, obtained by dividing through equation (1) by the number of moles of phase A $(m_{1A} + m_{2A} + m_{3A})$, equation (2) by the number of moles of phase B $(m_{1B} + m_{2B} + m_{3B})$, and so on. Substituting $\overline{S}_A = S_A/(m_{1A} + m_{2A} + m_{3A})$, $\overline{V}_A = V_A/(m_{1A} + m_{2A} + m_{3A})$, and $X_{1A} = m_{1A}/(m_{1A} + m_{2A} + m_{3A})$, and so on, and eliminating X_{3A} by virtue of the relation $X_{3A} = 1 - X_{1A} - X_{2A}$, we obtain

$$0 = \overline{S}_A dT - \overline{V}_A dP + X_{1A}(d\mu_1 - d\mu_3) + X_{2A}(d\mu_2 - d\mu_3) + d\mu_3 \quad (5)$$
$$0 = \overline{S}_B dT - \overline{V}_B dP + X_{1B}(d\mu_1 - d\mu_3) + X_{2B}(d\mu_2 - d\mu_3) + d\mu_3 \quad (6)$$
$$0 = \overline{S}_C dT - \overline{V}_C dP + X_{1C}(d\mu_1 - d\mu_3) + X_{2C}(d\mu_2 - d\mu_3) + d\mu_3 \quad (7)$$
$$0 = \overline{S}_D dT - \overline{V}_D dP + X_{1D}(d\mu_1 - d\mu_3) + X_{2D}(d\mu_2 - d\mu_3) + d\mu_3 \quad (8)$$

These equations can be solved, as Gibbs did (Gibbs, *op. cit.*, p. 98, eq. 129), by eliminating $d\mu_1$, $d\mu_2$, and $d\mu_3$ to obtain the slope of the univariant equilibrium on

a pressure-temperature diagram:

$$\frac{dP}{dT} = \frac{\begin{vmatrix} -\overline{S}_A & X_{1A} & X_{2A} & 1 \\ -\overline{S}_B & X_{1B} & X_{2B} & 1 \\ -\overline{S}_C & X_{1C} & X_{2C} & 1 \\ -\overline{S}_D & X_{1D} & X_{2D} & 1 \end{vmatrix}}{\begin{vmatrix} -\overline{V}_A & X_{1A} & X_{2A} & 1 \\ -\overline{V}_B & X_{1B} & X_{2B} & 1 \\ -\overline{V}_C & X_{1C} & X_{2C} & 1 \\ -\overline{V}_D & X_{1D} & X_{2D} & 1 \end{vmatrix}} \tag{9}$$

This equation does not provide much help in estimating the P-T conditions for the rock under consideration: the rock lies somewhere along the univariant curve on the P-T diagram, but its coordinates are not known. In order to obtain this information it is necessary to introduce the differentials of mineral composition dX_{1A}, dX_{2A}, and so on, as additional unknowns so that changes in mineral composition along the univariant curve can be followed. Thus, by measuring the composition of the minerals in the rock, its coordinates on the P-T diagram can be located. Equation (5) contains the quantities $(d\mu_1 - d\mu_3)$ and $(d\mu_2 - d\mu_3)$ that are functions of P, T, X_{1A}, *and* X_{2A}. The total differential of $d(\mu_1 - \mu_3)$ is equal to:

$$d(\mu_1 - \mu_3) = \left(\frac{\partial(\mu_1 - \mu_3)}{\partial T}\right)_{P,X_{1A},X_{2A}} dT + \left(\frac{\partial(\mu_1 - \mu_3)}{\partial P}\right)_{T,X_{1A},X_{2A}} dP + \left(\frac{\partial(\mu_1 - \mu_3)}{\partial X_{1A}}\right)_{P,T,X_{2A}} dX_{1A} + \left(\frac{\partial(\mu_1 - \mu_3)}{\partial X_{2A}}\right)_{P,T,X_{1A}} dX_{2A} \tag{10}$$

Substituting

$$\partial(\mu_1 - \mu_3)/\partial T = -(\overline{S}_{1A} - \overline{S}_{3A})$$
$$\partial(\mu_1 - \mu_3)/\partial P = (\overline{V}_{1A} - \overline{V}_{3A})$$
$$[\partial(\mu_1 - \mu_3)/\partial X_{1A}]_{P,T,X_{2A}} = [\partial(\partial\overline{G}_A/\partial X_{1A})_{P,T,X_{2A}}/\partial X_{1A}]_{P,T,X_{2A}} \equiv \overline{G}_{11A}$$

and

$$[\partial(\mu_1 - \mu_3)/\partial X_{2A}] = [\partial(\partial\overline{G}_A/\partial X_{1A})_{P,T,X_{2A}}/\partial X_{2A}]_{P,T,X_{1A}} \equiv \overline{G}_{12A}$$

$$(d\mu_1 - d\mu_3) = -(\overline{S}_{1A} - \overline{S}_{3A})dT + (\overline{V}_{1A} - \overline{V}_{3A})dP + \overline{G}_{11A}dX_{1A} + \overline{G}_{12A}dX_{2A} \tag{11}$$

$\overline{S}_{1A}$ is the partial molar entropy of component 1 in phase A and is related to $\overline{S}_A$ by the equation $\overline{S}_A = X_{1A}\overline{S}_{1A} + X_{2A}\overline{S}_{2A} + X_{3A}\overline{S}_{3A}$. $\overline{V}_{1A}$ is the partial molar volume of component 1 in phase A and is related to $\overline{V}_A$ by the equation $\overline{V}_A = X_{1A}\overline{V}_{1A} + X_{2A}\overline{V}_{2A} + X_{3A}\overline{V}_{3A}$. $\overline{G}_A$ is the molar Gibbs free energy of phase A and is related to μ_1, μ_2, and μ_3, by the equation $\overline{G}_A = X_{1A}\mu_1 + X_{2A}\mu_2 + X_{3A}\mu_3$. $\overline{G}_{11A}$, $\overline{G}_{22A}$, and $\overline{G}_{12A}$ are the principal and cross curvatures of the Gibbs free energy with respect to the two independent mole fractions X_{1A} and X_{2A}. Similarly for

$(d\mu_2 - d\mu_3)$ the resulting equation is

$$(d\mu_2 - d\mu_3) = -(\overline{S}_{2A} - \overline{S}_{3A})dT + (\overline{V}_{2A} - \overline{V}_{3A})dP + \overline{G}_{12A}dX_{1A} + \overline{G}_{22A}dX_{2A} \qquad (12)$$

Equations like these have been put to similar uses by Butler (1936, pp. 175–176), Morey (1936, pp. 251–254), and others. The present form of equations (11) and (12) was developed by Prof. J. B. Thompson, Jr., in class lectures at Harvard University; they are used by him in obtaining equations such as (13) and in studying the "Gibbs-Konovalow" theorems (cf. Prigogine and Defay, 1954, pp. 278–284). Considering equations (5)–(8) together with (11) and (12) it is to be noted that although two new unknowns have been added, two new equations have also been added; therefore, the variance of the system remains unchanged. There are six homogeneous equations in seven unknowns, and one can solve for

$$\frac{dX_{1A}}{dT} = \frac{\begin{vmatrix} -\overline{V}_A & X_{1A} & X_{2A} & 1 & -\overline{S}_A & 0 \\ -\overline{V}_B & X_{1B} & X_{2B} & 1 & -\overline{S}_B & 0 \\ -\overline{V}_C & X_{1C} & X_{2C} & 1 & -\overline{S}_C & 0 \\ -\overline{V}_D & X_{1D} & X_{2D} & 1 & -\overline{S}_D & 0 \\ (\overline{V}_{1A} - \overline{V}_{3A}) & -1 & 0 & 0 & (\overline{S}_{1A} - \overline{S}_{3A}) & \overline{G}_{12A} \\ (\overline{V}_{2A} - \overline{V}_{3A}) & 0 & -1 & 0 & (\overline{S}_{2A} - \overline{S}_{3A}) & \overline{G}_{22A} \end{vmatrix}}{\begin{vmatrix} -\overline{V}_A & X_{1A} & X_{2A} & 1 & 0 & 0 \\ -\overline{V}_B & X_{1B} & X_{2B} & 1 & 0 & 0 \\ -\overline{V}_C & X_{1C} & X_{2C} & 1 & 0 & 0 \\ -\overline{V}_D & X_{1D} & X_{2D} & 1 & 0 & 0 \\ (\overline{V}_{1A} - \overline{V}_{3A}) & -1 & 0 & 0 & \overline{G}_{11A} & \overline{G}_{12A} \\ (\overline{V}_{2A} - \overline{V}_{3A}) & 0 & -1 & 0 & \overline{G}_{12A} & \overline{G}_{22A} \end{vmatrix}} \qquad (13)$$

Equation (13) emphasizes that if a rock containing a univariant equilibrium of minerals is found, the composition of only one of its minerals need be measured in order to estimate the *P-T* conditions under which it originated, provided that the necessary thermodynamic data on $\overline{V}_A$, $\overline{S}_A$, and so on, are available. Equations such as (13) can be obtained for the variation in composition of any other mineral participating in the equilibrium by including pairs of equations similar to (11) and (12) written for the mineral of interest.

Quantitative utilization of equations such as (13) is necessarily dependent on adequate thermodynamic data. The volume terms $\overline{V}_A$, $\overline{V}_{1A}$, and so on, are much better known than the other quantities because they can be measured relatively easily using X-ray diffraction or other techniques (Robie *et al.*, 1966; Skinner, 1966; Birch, 1966). The entropy terms, $\overline{S}_A$, $\overline{S}_{1A}$, and so on, and the Gibbs' free energy terms $\overline{G}_A$, $\overline{G}_{11A}$, and so on, are less well known because it is more difficult to measure them. Data of this kind are most abundant for minerals of fixed chemical composition (Robie and Waldbaum, 1968). Data on the mixing properties of solid solutions from which terms such as $\overline{S}_{1A}$ and $\overline{G}_{11A}$ can be evaluated are currently being gathered by analysis of experimentally determined phase dia-

grams (Thompson and Waldbaum, 1968, 1969) and by calorimetric techniques (Waldbaum and Robie, 1971).

Divariant Equilibria

Consider the divariant equilibrium of minerals A, B, and C in the same ternary system considered above. In this example equations (5)–(7), (11), and (12) are applicable. Two equations of the type (11) and (12) written for mineral B can be added. There are seven equations and nine unknowns for this system of homogeneous equations. Solutions such as (9) and (13) can be obtained by assigning a constant value to one of the unknowns so that d(unknown) = 0. For petrological purposes, it is necessary to obtain two solutions, first with X_{1A} = constant and second with X_{1B} = constant.

$$\frac{\partial P}{\partial T}_{(X_{1A} = \text{constant})} = \frac{\begin{vmatrix} -\bar{S}_A & X_{1A} & X_{2A} & 1 & 0 \\ -\bar{S}_B & X_{1B} & X_{2B} & 1 & 0 \\ -\bar{S}_C & X_{1C} & X_{2C} & 1 & 0 \\ (\bar{S}_{1A} - \bar{S}_{3A}) & -1 & 0 & 0 & \bar{G}_{12A} \\ (\bar{S}_{2A} - \bar{S}_{3A}) & 0 & -1 & 0 & \bar{G}_{22A} \end{vmatrix}}{\begin{vmatrix} -\bar{V}_A & X_{1A} & X_{2A} & 1 & 0 \\ -\bar{V}_B & X_{1B} & X_{2B} & 1 & 0 \\ -\bar{V}_C & X_{1C} & X_{2C} & 1 & 0 \\ (\bar{V}_{1A} - \bar{V}_{3A}) & -1 & 0 & 0 & \bar{G}_{12A} \\ (\bar{V}_{2A} - \bar{V}_{3A}) & 0 & -1 & 0 & \bar{G}_{22A} \end{vmatrix}} \quad (14)$$

and

$$\frac{\partial P}{\partial T}_{(X_{1B} = \text{constant})} = \frac{\begin{vmatrix} -\bar{S}_A & X_{1A} & X_{2A} & 1 & 0 \\ -\bar{S}_B & X_{1B} & X_{2B} & 1 & 0 \\ -\bar{S}_C & X_{1C} & X_{2C} & 1 & 0 \\ (\bar{S}_{1B} - \bar{S}_{3B}) & -1 & 0 & 0 & \bar{G}_{12B} \\ (\bar{S}_{2B} - \bar{S}_{3B}) & 0 & -1 & 0 & \bar{G}_{22B} \end{vmatrix}}{\begin{vmatrix} -\bar{V}_A & X_{1A} & X_{2A} & 1 & 0 \\ -\bar{V}_B & X_{1B} & X_{2B} & 1 & 0 \\ -\bar{V}_C & X_{1C} & X_{2C} & 1 & 0 \\ (\bar{V}_{1B} - \bar{V}_{3B}) & -1 & 0 & 0 & \bar{G}_{12B} \\ (\bar{V}_{2B} - \bar{V}_{3B}) & 0 & -1 & 0 & \bar{G}_{22B} \end{vmatrix}} \quad (15)$$

Equations (14) and (15) can be used to contour mineral compositions in the divariant region of the P-T diagram where minerals A, B, and C, are in equilibrium. Each equation produces a set of isopleths, and the two sets of contours form an intersecting grid on the P-T diagram. The P-T conditions for the formation of a rock containing a divariant mineral equilibrium can be estimated simply by measuring the compositions of two of the minerals and locating the P-T coordinates where their appropriate isopleths intersect. The theoretical basis for the petrogenetic grids of O'Hara (1967, p. 396, Fig. 12), Hensen and Green (1973, p. 154, Fig. 2; p. 155, Fig. 3), and Boyd (1973) is provided by equations such as (14) and (15).

The Fluid Phase of Metamorphic Rocks

An additional problem, not discussed previously, is encountered in the applica-

tion of the phase rule to metamorphic rocks. Metamorphic rocks are derived from sediments or igneous rocks by deep burial and consequent recrystallization (regional metamorphism) or by heating caused by intrusive igneous rocks (contact metamorphism). The problem in application of the phase rule arises because the aqueous fluid likely to have been present before and during metamorphism is almost completely expelled from the rock by recrystallization. The fluid phase of metamorphic rocks is not readily accessible to direct chemical analysis, and therefore, its components can only be enumerated by assumption. Thus, the true variance of a given mineral equilibrium is in doubt. Furthermore, estimates of the P-T conditions of rock recrystallization based on mineral equilibria in which fluid species participate are likely to be affected by variations in the properties of the fluid phase. One method of attacking this problem is to study equilibria of minerals that contain substances that are likely to be components of the fluid phase. The compositions of such minerals are readily measurable quantities that are functions of the composition of the fluid phase as well as temperature and pressure. In order not to become entangled by simultaneous consideration of geothermometry, geobarometry, and the fluid phase, it is necessary to select a regionally metamorphosed outcrop area containing as wide a variety as possible of different rock types and different mineral equilibria, but small enough (say 10 m $\times$ 10 m) so that it can be assumed the rocks experienced uniform pressure and temperature at any given time during metamorphism. In this experiment T and P can be set to constant values ($dT = dP = 0$), and it is possible to study the behavior of the fluid phase as it is expressed by trivariant and quadrivariant mineral equilibria.

Trivariant Equilibria

Consider the trivariant five-component system SiO_2-Al_2O_3-FeO-MgO-H_2O containing the four minerals quartz (SiO_2), kyanite (Al_2SiO_5), staurolite ($Fe_4Al_{18}Si_8O_{48}H_2 - Mg_4Al_{18}Si_8O_{48}H_2$), and chloritoid ($Fe_2Al_4Si_2O_{10}(OH)_4 - Mg_2Al_4Si_2O_{10}(OH)_4$). In addition to the four Gibbs-Duhem equations written with the components indicated in parentheses, we have the following conditions of equilibrium: one condition of Fe-Mg exchange equilibrium between staurolite and chloritoid

$$d(\mu_{Fe_4Al_{18}Si_8O_{48}H_2} - \mu_{Mg_4Al_{18}Si_8O_{48}H_2}) = 2d(\mu_{Fe_2Al_4Si_2O_{10}(OH)_4} - \mu_{Mg_2Al_4Si_2O_{10}(OH)_4}) \tag{16}$$

one condition of heterogeneous equilibrium between μ_{H_2O}, a likely component species of the fluid, and the components of the minerals

$$d(3\mu_{H_2O}) = d(5\mu_{Al_2SiO_5} + 2\mu_{Mg_2Al_4Si_2O_{10}(OH)_4} - 1\mu_{SiO_2} - 1\mu_{Mg_4Al_{18}Si_8O_{48}H_2}) \tag{17}$$

There is also one of the equations of type (11) written for a binary solution:

$$d(\mu_{Fe_4Al_{18}Si_8O_{48}H_2} - \mu_{Mg_4Al_{18}Si_8O_{48}H_2}) = -(\overline{S}_{Fe,St} - \overline{S}_{Mg,St})dT + (\overline{V}_{Fe,St} - \overline{V}_{Mg,St})dP + \overline{G}_{FeFe,St}dX_{Fe,St} \tag{18}$$

where the subscript "Fe" refers to the Fe component of staurolite and the subscript "St" refers to the mineral staurolite. Taking into account equations (14–18), the four Gibbs-Duhem equations, and the conditions $dT = dP = 0$, we can solve for

$$\left(\frac{\partial \mu_{H_2O}}{\partial X_{Fe,St}}\right)_{P,T} = \overline{G}_{FeFe,St}(X_{Fe,St} - X_{Fe,Ct})/3 \tag{19}$$

where the subscripts are as above and "Ct" refers to the mineral chloritoid. This equation demonstrates that the properties of a probable component of the fluid phase can be measured simply by analyzing the compositions of two minerals. Because of the binary nature of the solid solutions considered, it is possible to evaluate the sign of the right-hand side of equation (19) without restrictive assumptions as to the solution properties of the minerals. The quantity $\overline{G}_{FeFe,St}$ is necessarily positive for a stable binary solution (Gibbs, *op. cit.*, pp. 100–115, 129–134); therefore, the sign of (19) is determined by the relative magnitudes of the easily measured quantities $X_{Fe,St}$ and $X_{Fe,Ct}$. If thermodynamic data are available on the mixing properties of the binary solution of interest, and if reasonable estimates of P and T can be made, equations such as (19) can be used to measure the magnitude of chemical potential gradients between rocks which have the same mineral assemblage but whose minerals have different compositions. In the absence of data on the mixing properties of solid solutions an order-of-magnitude calculation can be made using the ideal solution model. Differentiating

$$\overline{G}_A = X_{1A}\mu_1{}^0 + X_{2A}\mu_2{}^0 + RT(X_{1A} \ln X_{1A} + X_{2A} \ln X_{2A})$$

twice with respect to the independent mole fraction X_{1A} the following result is obtained:

$$\left(\frac{\partial^2 \overline{G}_A}{\partial X_{1A}{}^2}\right)_{P,T} = \frac{RT}{X_{1A} - X_{1A}{}^2}$$

where R is equal to the universal gas constant and $\mu_1{}^0$ and $\mu_2{}^0$ are the chemical potentials of pure components 1 and 2 at the P and T of interest.

Quadrivariant Equilibria

Consider the quadrivariant seven-component system SiO_2-TiO_2-Al_2O_3-Fe_2O_3-FeO-MgO-H_2O containing the five minerals quartz, kyanite, chloritoid, ilmenite ($FeTiO_3$-Fe_2O_3), and staurolite

$$(Fe_4Al_{18}Si_8O_{48}H_2\text{-}Mg_4Al_{18}Si_8O_{48}H_2\text{-}Fe_4Al_{18}Ti_8O_{48}H_2).$$

In this example, staurolite is a ternary solution with Fe-Mg and Si-Ti substitution. In addition to the five Gibbs-Duhem equations we have equation (16), equation (17), one condition of heterogeneous equilibrium between the Ti components of ilmenite and staurolite,

$$d(8\mu_{FeTiO_3}) = d(8\mu_{SiO_2} - 8\mu_{Al_2SiO_5} + 4\mu_{Fe_2Al_4Si_2O_{10}(OH)_4} - \mu_{Fe_4Al_{18}Si_8O_{48}H_2} + \mu_{Fe_4Al_{18}Ti_8O_{48}H_2} - 8\mu_{H_2O}) \quad (20)$$

one condition of equilibrium between μ_{O_2}, a probable component of the fluid phase, and the components of the minerals,

$$d(\mu_{O_2}) = d(4\mu_{Al_2SiO_5} - 2\mu_{Fe_2Al_4Si_2O_{10}(OH)_4} + 2\mu_{Fe_2O_3} - 4\mu_{H_2O}) \quad (21)$$

and two equations of type (18) for chloritoid and ilmenite. Taking into account the conditions $dT = dP = 0$, $dX_{Fe_2O_3,ilmenite} = 0$, and the eleven equations one can solve for

$$\left(\frac{\partial \mu_{H_2O}}{\partial \mu_{O_2}}\right)_{P,T,X_{Fe_2O_3,ilmenite}} = \frac{(4X_{Fe,Ct} - 2)X_{Fe_4Al_{18}Ti_8O_{48}H_2,St} + (2X_{Fe_4Al_{18}Si_8O_{48}H_2,St} - 2X_{Fe,Ct})}{(8X_{Fe_4Al_{18}Si_8O_{48}H_2,St} + 8X_{Fe_4Al_{18}Ti_8O_{48}H_2,St} - 2X_{Fe,Ct} - 6)} \quad (22)$$

and, with $dX_{Fe,Ct} = 0$,

$$\left(\frac{\partial \mu_{H_2O}}{\partial \mu_{O_2}} \right)_{P,T,X_{Fe,Ct}} = \frac{8X_{Fe_2O_3,ilmenite}X_{Fe_4Al_{18}Ti_8O_{48}H_2,St}}{(16X_{Fe_4Al_{18}Ti_8O_{48}H_2,St} + 16X_{Fe_2O_3,ilmenite}X_{Fe_4Al_{18}Ti_8O_{48}H_2,St} + 6X_{Fe_2O_3,ilmenite} - 6)} \quad (23)$$

Equations (22) and (23) define contours of mineral composition in the quadrivariant regions of an isothermal, isobaric $\mu_{H_2O} - \mu_{O_2}$ diagram in the same way that equations (14) and (15) defined contours in the divariant regions of a P-T diagram. The $\mu_{H_2O} - \mu_{O_2}$ conditions of recrystallization for a given quadrivariant mineral equilibrium can be estimated by analyzing the compositions of two of its minerals, locating the intersection of the appropriate isopleths on a $\mu_{H_2O} - \mu_{O_2}$ diagram and reading off the coordinates of the intersection. Equations (22) and (23) contain readily measurable composition terms on their right-hand sides. These equations imply that the rocks themselves contain all the information needed to deduce relative differences in chemical potentials of volatile components between adjacent mineral assemblages. Thermodynamic data are required, of course, to calibrate the quadrivariant $\mu_{H_2O} - \mu_{O_2}$ grid to specific numerical values of μ_{H_2O} and μ_{O_2}.

The treatment of trivariant and quadrivariant equilibria given here differs from the conventional form but is compatible with it. The method used by Korzhinskii (1959) and others in analyzing μ_i vs. μ_j diagrams is best suited to studying minerals of fixed chemical composition. The method described above is well suited to the study of mineral solid solutions of variable composition.

Conclusion

The analytical method developed by Gibbs in deriving the phase rule remains today one of the most powerful means of formulating phase equilibria. It is hoped that the examples discussed in this review adequately demonstrate to the reader how to apply this method to rocks and what kinds of information may be gained from its application. One advantage of the method is that the equations are derived in the most general way with a minimum of assumptions. Mineral assemblages containing any number of phases and components may be so treated. The method is well suited to the study of mineral assemblages containing solid solutions of variable chemical composition. When assumptions are made in order to evaluate numerically the equations, they have to be introduced explicitly and their consequences are immediately obvious. Full realization of the potential of Gibbs' method must await experimental measurement of additional thermodynamic data, especially with regard to the mixing properties of solid solutions. The existence of Gibbs' method and the quality of the information on the conditions of rock formation that it can provide should act as an inspiration to experimental petrologists to redouble their efforts to acquire basic thermodynamic data.

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A Biologist's View of Gibbs' Contributions

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ABSTRACT

Five examples illustrate the particular orientation given by Gibbs' thermodynamics or statistical mechanics to approaches to contemporary research in the life sciences.

Approaching the hundredth anniversary of Gibbs' work on heterogeneous equilibrium, and trying to assess Gibbs' influence on biology arouses feelings of nostalgia. I must therefore introduce my topic with a few personal notes. I first learned of Gibbsian thermodynamics in Leigh Page's course on Introduction to Theoretical Physics, where I was introduced to the Ψ, χ, ζ functions of the Professor. So while the rest of the world talked of enthalpies, free energies, and the rest, we contented ourselves with the original 1875 nomenclature. This quaint approach was not so strange, for Page came to Yale in 1900 and spent the first 3 years of his career in a department in which Gibbs was still an active contributor. Thus, I feel myself a member in a line of direct microcanonical succession from the master himself.

These psychological ties go deeper, for on several occasions when my unease over a scientific problem rendered me peripatetic I have wandered from the Gibbs laboratory where I work to the nearby Grove Street Cemetery to pause before the Gibbs family gravesite and wonder where this modest man drew the inspiration to penetrate problems with such clear and brilliant insight. However, lest we get maudlin, let us turn from the man to his work.

Gibbsian concepts have so penetrated modern biological and biochemical thought that any attempt to survey their impact would go far beyond the limits of this presentation. Rather, we will focus attention on 5 examples from contemporary life science, where the particular orientation given by Gibbs to some aspect of thermodynamics or statistical mechanics has motivated a significant approach to some biological area.

One of the most fertile papers in modern molecular biology has been "Equilibrium Sedimentation of Macromolecules in Density Gradients" by Meselson et al. (1957). These studies opened the way to the separation of isotopically labeled nucleic acids on the basis of density. It provided immediately successful analytical and separatory techniques which have been among the principle tools in the study of both prokaryotic and eukaryotic genetic material. The application of this technique quickly led to an understanding of the semiconservative replication of bacterial DNA (Meselson and Stahl, 1958). Much of our detailed understanding of the nature of DNA replication rests on experiments using equilibrium density gradients.

The theory of density gradients follows from the generalization of the chemical potential set forth by Gibbs in his study of heterogeneous equilibrium. Thus the theory section of the density gradient paper begins:

"II Quantitative Relations

The total potential of any component at equilibrium in a closed system at constant temperature must be uniform through the system. In a centrifugal

field this requirement results in the rigorous condition.[3]

$$M_i(1 - \bar{v}_i\rho(r))\omega^2 rdr - \sum_k \frac{\partial \mu_i}{\partial C_k} dC_k = 0$$

where M_i, $\bar{v}_i$, μ_i, C_i are molecular weight, partial specific volume, partial molar (Gibbs) free energy and concentration of the ith component.''

Reference 3 in the above quotation is to T. Svedberg and K.O. Pedersen, *The Ultracentrifuge* (1940). Interestingly enough, Pedersen in his chapter on sedimentation equilibrium in the reference just cited has a footnote which states: ''For the thermodynamical deduction of equations for sedimentation equilibrium compare, for instance: The Scientific Papers of J. W. Gibbs *1*, 144–50 (London 1906); etc, etc.'' The reference in Gibbs is to a section called ''The Conditions of Equilibrium for Heterogeneous Masses under the Influence of Gravity.'' The equation given by Meselson et al. (1957) follows directly from the treatment given by Gibbs with the substitution of the centrifugal acceleration $\omega^2 r$ for the gravitational acceleration. We have thus traced the direct link from the cesium chloride gradients for DNA back to the underlying theory as set forth in 1875.

The second example I would like to give has provided a central theme to modern theoretical ecology and has formed the underlying assumption of a number of studies. The paper I refer to is ''A Statistical Mechanics of Interacting Biological Species'' by Edward Kerner (1957). Kerner is a physicist and an unabashed Gibbsian who has used the insights of statistical mechanics to attempt to deal with biological problems of such high order of complexity that detailed treatment seems to be impossible. Kerner's approach has stimulated theoretical studies in cell biology (Goodwin, 1963), neurophysiology (Cowan, 1972), and complex chemical systems (Kerner, 1964).

Kerner's reliance on Gibbs' statistical mechanics is most clearly seen in the following quotation from the 1957 paper:

''To proceed at once from the starting Volterra equations is therefore plainly desirable, if not absolutely necessary. Now, the statistical mechanics customary in physics, that form of it elaborated by J. W. Gibbs (1902), rests on the Hamiltonian form of the equations of motion only weakly, almost incidentally, the role of Hamilton's equations being to make evident the two corner stones of the statistical development: Liouville's theorem and energy conservation. It will appear that the initial Volterra equations readily admit a Liouville's theorem and a universal constant of the ''motion'' somewhat like the Hamiltonian of classical dynamics; and then a statistical analysis of some simplicity, parallel to Gibbs', becomes feasible. Herewith we find a lesson from physics as well as to physics, an example of how much broader is the statistical side of statistical mechanics than the mechanics which calls it into existence.''

With the above in mind Kerner proceeds to recast the Volterra equations into equations of motion and then to form a ''Gibbs ensemble of biological associations.'' For such an ensemble he proves ''Liouville's theorem of conservation of density in phase.'' The analogy is carried forward to the introduction of a number of new parameters which have provided stimulus to ecological thought.

The statistical ideas of Gibbs stressed by Kerner had already entered biology by an entirely alternate route. Norbert Wiener in his book *Cybernetics* (1948) opened up a whole new paradigm on the role of feedback systems in biology. Chapter II of that book deals extensively with the work of Gibbs and Henri Lebesque. Wiener wrote:

''Gibbs, mathematician that he was, always regarded mathematics as ancillary to physics. Lebesque was an analyst of the purest type, an able exponent of the extremely exacting modern stand-

ards of mathematical rigor; . . . Nevertheless, the work of these two men forms a single whole in which the questions asked by Gibbs find their answers, not in his own work, but in the work of Lebesque."

Wiener proceeds to a detailed discussion of the ergodic hypothesis which serves as an introduction to his discussion of information. Thus the cybernetic and information theory approaches in biology are also touched by a Gibbsian influence.

Gibbsian statistical mechanics, insofar as it provided part of the foundation of information theory, has had a broad and difficult task to delineate influence on contemporary biology. The information measure of Shannon, which relates closely to the entropy measure of statistical mechanics, has been widely used by biologists in a series of problems ranging from evaluating primary sequence in biopolymers to behavioral studies dealing with the rate of information processing in humans (Quastler, 1953).

The next example is much more experimental in outlook, yet illustrates the power of one of the concepts from the discussion of heterogeneous equilibrium. For a number of years the detailed study of membrane structure has been a problem of great difficulty. The obvious chief role of the plasma membrane is to separate the aqueous interior of the cell from the aqueous exterior. Such separation is necessary to accord an integrity to the interior, so that a degree of regulation is possible. Since all membranes contain appreciable amounts of amphiphilic molecules such as phospholipids, these substances were some of the logical candidates for the non-aqueous phase. The very notion of phase in this sense goes back to the section of the heterogeneous equilibrium paper called "On Coexistent Phases of Matter."

The notion that membranes consist largely of bimolecular leaflets of polar lipid molecules with interior lipid and exterior polar groups has been one of the popular membrane models since late in the 1920's. However, proof was lacking. In 1969 Stein and coworkers (Stein et al., 1969) examined purified membranes of *Acholeplasma laidlawii* by differential scanning calorimetry and observed a distinct heat absorption peak which they took to be associated with a phase transition in the membrane bilayer. They extracted the membrane lipids, formed synthetic bilayers from them, and observed that these materials yielded differential calorimetric scanning patterns similar to the membrane.

In our laboratory we have repeated Stein's results and have been able to utilize Dr. Sturtevant's highly sensitive calorimeter to demonstrate the phase transition in whole living cells (Melchior et al., 1970). Engelman (1970) used the phase transition to measure the change of bilayer thickness during the phase transition by the technique of x-ray diffraction. The phase change and change of thickness aided in interpreting the diffraction pattern. This series of phase change measurements has now made it very clear that most of the lipids in mycoplasma membranes are in the bimolecular leaflet configuration.

Our final case study involves an area where the answers are not yet completely in—the attempts to develop a nonequilibrium thermodynamics and statistical mechanics appropriate to biology. An example of this approach is the book "Nonequilibrium Thermodynamics in Biophysics" by Aharon Katchalsky and Peter F. Curran (1965). They start with the introduction of the Gibbs equation

$$dU = TdS - PdV + fdl + \psi de + \sum_{i=1}^{k} \mu_i dn_i + \ldots$$

which is strictly applicable to equilibrium reactions and extend it to near equilibrium by the assumption of local equilibrium. They note "the range of applicability of the Gibbs equation cannot be specified on *a priori* grounds, and the justification of its use rests in the final analysis, on the validity of the

results obtained. On the basis of this criterion, many irreversible processes of interest can be treated using Eq. (7-2) as a starting point." Using this approach, the authors develop an approach to membrane and transport processes which has been influential in the current study of these problems in biology.

The possible treatment of nonequilibrium cases has been pursued in statistical mechanics as well as thermodynamics. E. T. Jaynes in his paper "Information Theory and Statistical Mechanics" (1957) has extended the statistical mechanics of Gibbs to make contact with the information theory of Shannon and Weaver (1964) and has provided a basis for extending the ensemble approach into the nonequilibrium domain. Jaynes has written:

"The mathematical facts concerning maximization of entropy were pointed out long ago by Gibbs. In the past, however, these properties were given the status of side remarks not essential to the theory and not providing in themselves any justification for the methods of statistical mechanics. The feature which was missing has been supplied only recently by Shannon in the demonstration that the expression for entropy has a deeper meaning, quite independent of thermodynamics. This makes possible a reversal of the usual line of reasoning in statistical mechanics. Previously, one constructed a theory based on the equations of motion, supplemented by additional hypotheses of ergodicity, metric transitivity, or equal *a priori* probabilities, and the identification of entropy was made only at this end, by comparison of the resulting equations with the laws of phenomenological thermodynamics. Now, however, we can take entropy as our starting concept, and the fact that a probability distribution maximizes the entropy subject to certain constraints becomes the essential fact which justifies use of that distribution for inference."

In the Jaynes formalism the maximization of entropy is the equivalent to making the maximally noncommittal statement with respect to missing information. It is an extension of Laplace's principle of insufficient reason to any case where constraints can be formulated and a probability distribution is a reasonable way to formulate the problem. It is therefore applicable to any situation where ensembles and ensemble averages are appropriate for the formulation of a problem.

We have attempted to use the Jaynes approach in 3 studies aimed at developing non-equilibrium statistical techniques for biological problems (Rider and Morowitz, 1968; Morowitz, 1971; Corbet and Morowitz, 1972). While such approaches are in their early stages, they do indicate the potential power of the ensemble technique in problems extending beyond the original equilibrium mechanics treated by Gibbs.

In retrospect it is surprising to realize the number of crucial areas in modern biological thought where concepts originally set forth by Josiah Willard Gibbs have come to maturity. It requires an event such as this symposium to force us to focus attention on the fruits of the labors of a modest genius such as Gibbs.

As a biologist, one can only lament that Gibbs failed to make a contribution to the gene pool. While he left no children, he has numerous intellectual heirs who share the huge wealth of ideas left deposited in his writings.

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Centennial of Gibbs' Thermodynamics— Concluding Remarks

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The President's reference to the 18 years that have elapsed since I held the position he now occupies is one of two recent instances that have reminded me of the rapid passage of time. The other occurred a few days ago when, in preparation for this symposium, I looked into the volumes of Gibbs' collected works that I first studied some 45 years ago. Although I had remembered vividly the pleasure that came from following the closely knit arguments and ingenious graphical demonstrations that led him from two very simply stated but pregnant general principles to elucidate phenomena and laws covering a vast area of physical chemistry, I found with some dismay that much of the content seemed unfamiliar, forgotten over years of preoccupation with other matters. Only handwritten marginal notes assured me that once I had studied these papers assiduously. The papers tonight brought alive again the realization that those articles published—mostly in the Transactions of the Connecticut Academy approximately a century ago—are still a rich mine of scientific gold.

Let me base these concluding remarks on a passage from a memorial biography[1] written in 1903 by H. A. Bumstead, a student and colleague of Gibbs, and later Professor of Physics and Director for many years of the Sloane Physics Laboratory at Yale:

> "Although he disregarded many of the shibboleths of the mathematical rigorists, his logical processes were really of the most severe type; in power of deduction, of generalization, in insight into hidden relations, in critical acumen, utter lack of prejudice, and in the philosophical breadth of his view of the object and aim of physics, he has had few superiors in the history of the sciences; and no student could come in contact with this serene and impartial mind without feeling profoundly its influence in all his future studies of nature."

Through the exercise of these *logical processes*, Gibbs left a legacy of elegant instruments of thought such as the concept of the chemical potential (often very loosely called the "Free Energy" or "Partial Free Energy") together with

[1]American Journal of Science, Series 4, Vol. XV, September 1903. Reprinted in "The Collected Works of J. Willard Gibbs, Vol. I, Longmans, Green & Co., New York, 1928.

equations and diagrams using this concept. Dr. Rumble has shown how these methods, combined with a knowledge of the components present in a geochemical system, can enable the ingenious investigator to thread his way through the maze of complicated reactions taking place in the formation of metamorphic rocks and deduce the conditions under which they were formed, thereby strengthening our knowledge of the thermal history of the earth's crust. Dr. Mountain has shown us that the graphical expressions logically derived by Gibbs to describe, among other things, the thermodynamics of critical phenomena have relevance not only to the classical cases but also to "phase" transitions which are some of the most exciting phenomena in modern solid state physics.

Gibbs' *insight into hidden relations* implicit in simple, empirically established general principles is a feature of "On the Equilibrium of Heterogeneous Substances" that strikes the attention of any reader. In following his logical arguments relentlessly to take into account all imaginable variables that might influence a system, such as *gravity, capillarity, and electromotive force*, Gibbs formulated relationships that over the years have assumed more and more general significance. Dr. Morowitz has laid before us many examples illustrating clearly the direct and indirect consequences in the fundamental exploration of biological systems of "hidden relations" uncovered by Gibbs.

Taken together, the discussions we have heard tonight amply endorse Bumstead's assessment of the surpassing *philosophical breadth of his (Gibbs') view of the object and aim of physics*.

Although this symposium is primarily concerned with the thermodynamics of Gibbs, I can not refrain from a further remark inspired by a statement made by Dr. Morowitz concerning Gibbs' studies of statistical mechanics to the effect that the mathematical statistical structure Gibbs developed now transcends in importance the mechanics that have been incorporated into it.

The following extract[2] from the preface to "Elementary Principles in Statistical Mechanics" is illuminating:

" . . . Even if we confine our attention to the phenomena distinctively thermodynamic, we do not escape difficulties in as simple a matter as the number of degrees of freedom of a diatomic gas. It is well known that while theory would assign to the gas six degrees of freedom per molecule, in our experiments on specific heat, we cannot account for more than five. Certainly, one is building on an insecure foundation, who rests his work on hypotheses concerning the constitution of matter.

"Difficulties of this kind have deterred the author from attempting to explain the mysteries of nature, and have forced him to be contented with the more modest aim of deducing some of the more obvious propositions relating to the statistical branch of mechanics. Here, there can be no mistake in regard to the agreement of the hypotheses with the facts of nature, for nothing is assumed in that respect. The only error into which one can fall, is the want of agreement between the premises and the conclusions, and this, with care, one may hope, in the main, to avoid."

The choice and definition of a subject to which he is to devote his time and effort for an indefinite period is the most important decision a scientific investigator makes. The problem must be significant, material for its solution must be at least foreseeable, and the solution must be within, indeed call for, the highest intellectual powers of the investigator. The ability of a person to assess these conditions and choose appropriate problems proclaims the genius of the investigator and separates the first class from the nth class scientist.

These criteria are compatible with Gibbs' rank as a first class natural philosopher, although, if confronted with his proposal, it is not so certain that "blue ribbon" panels today would have supported his effort.

Tonight's symposium brings out clearly that the thinking of Gibbs has had a lasting influence on the theoretical and experimental development of all areas of modern science.

Let me now call attention to an area of human activity where the example of

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[2] "The Collected Works of J. Willard Gibbs, Vol. II, Longmans, Green & Co., New York, 1928, p. x (preface).

Gibbs' life and work has considerable relevance but which now goes on as if he and other thinkers had not existed. You will recall that the series of papers "On the Equilibrium of Heterogeneous Substances"[3] began with the following words:

> " *'Die Energie der Welt ist constant. Die Entropie der Welt strebt einem Maximum zu.'*
>
> —*Clausius*
>
> The comprehension of the laws which govern any material system is greatly facilitated by considering the energy and entropy of the system in the various states of which it is capable."

On the basis of these empirical principles Gibbs gives as the criterion for a system (isolated) to be in equilibrium, any change in the state of the system must be such that $(\delta S)_E \leqq 0$, where S is the entropy and E the energy of the system. Without introduction of energy from without the system in equilibrium has no potential for yielding useful work and is dead. In moving towards this state of equilibrium, the system undergoes a number of spontaneous changes in which the useful work given out under specific conditions may be expressed as $W = \Delta E - T\Delta S$. If these changes take place *reversibly*—i.e., they are held in control by a mechanism that constrains them to take place under equilibrium conditions, slowly enough, the work that can be obtained approaches its maximum value.

If, on the other hand, these changes are uncontrolled, highly irreversible, the useful work obtained for a given energy change is much less than the maximum obtainable, indeed sometimes close to 0, the energy being all absorbed by $T\Delta S$, the system rushes rapidly and wastefully to the equilibrium state where the entropy takes on its maximum value. For example, if we burn hydrogen and oxygen in a flame no useful work results, with an intermediate device such as a steam engine we may obtain some useful work at the expense of the entropy increase, with a fuel cell we may control the speed of the hydrogen oxygen reaction by sensing and adjusting the electromotive force of the cell in a way that brings the useful work yielded as close to the maximum as we wish. The human body is well supplied with control mechanisms which sense departure from equilibrium conditions and control the processes taking place to use most economically the energy intrinsic in the food we eat and the air we breathe.

Today, we are in the midst of a so-called "energy crisis." The word "energy" is used by the propagandists of all kinds to lend an aura of scientific dignity in making capital out of a situation in which the demand for certain non-renewable (or rather difficultly renewable) resources exceed the supply, fuel now and metals soon being the chief items of concern. If one has to use a word, well defined in science but not well understood elsewhere, it might be better to describe the current situation as an "entropy crisis". This term would at least cover three important current problems and emphasize the close relationship between them. For the current fuel or "free energy" crisis that commands so much public attention—written and spoken words today—the pollution or waste problem that held the center of the stage yesterday, and the materials problem which will arouse as much concern tomorrow; all arise from a common source, uncontrolled dispersal of resources—runaway increase of entropy, to state the matter succinctly if loosely.

I have spoken of the role of control systems in regulating the processes whereby nature permits us to extract from the resources[4] assembled over millions of years "useful work" and its

[3]Transactions of the Connecticut Academy, III, pp. 108–248, Oct. 1875–May, 1876, and pp. 343–524, May 1877–July 1878.

[4]Looking at "non renewable" resources such as fossil fuels, metallic ore concentrations, we may consider the earth to be an isolated system. However, as regards "renewable" resources such as water power, farm and forest products, the earth is not an isolated system; the energy received from the sun continually reducing the entropy, increasing the potential of part of the system to yield useful work.

social equivalent, commodities, services, and even luxuries.[5] Through intellects like that of Gibbs, man has learned that there are fundamental limits to these processes and, within these limits, how to realize the maximum benefits from them.

The resources of the earth are vast but not unlimited, and it is high time that this knowledge was used to greater social effect, but to do so may require very fundamental changes in our thoughts and communications. An essential feature of the control of any process or machine is "negative feed back control." A sensor gives *timely* and *accurate* information concerning the present magnitude and trend of the output of the process or machine; in a comparator this information is compared with what the magnitudes and trends should be to preserve stable and efficient operations, and differences are converted into information signals sent to change the input of the machine to *oppose* undesirable trends in its operation. Thus stability is maintained. If because information concerning the output is *untimely, inaccurate*, or the system is deliberately so designed, increase in the output may result in *increase* in the input, and the control system reinforces rather than opposes trends in the system's operation. This is known as "positive feed back" and results in exponential growth of the speed of the output of the process or machine. Explosions, be they in bombs, population, pollution or knowledge, all involve "positive feed backs." Such processes are irreversible, large amounts of energy being required to restore the system to its original state.

Social systems may be regarded as complex, dynamic and sensitive machines, designed to promote survival in a hostile environment of the system itself and of its individual components. Their successful operation depends on the sophistication and reliability of their control systems to conduct operations in accordance with the laws of nature, in particular the laws of thermodynamics. In democratic societies the input of the social machine is basically through the motivation, thought and action of the individual, the output is group behavior, concerted effort to optimize the creation of intellectual and material products needed to support the life and welfare of the system. The control system in a modern society is very complex. There are many sensors detecting and predicting the outputs and trends of the system—public servants, writers, philosophers, and religious leaders, to give some examples. The "information" generated by these sensors may or may not be timely and accurate, and, when processed through the comparator furnished with the corporate memory of history, yields control signals of varying and often unknown quality, often being tinged with the human tendency to dramatic exaggeration. The channels communicating these signals to the input also introduce the noise of untimeliness, uncertainty and error. Thus the signals, arriving at the input of the machine are very noisy, the negative feed backs so necessary to stability often being submerged by stronger messages causing positive feed backs and introducing hysteria or crisis psychology into the motivation of the individual.

This is not at all an ideal state of affairs and I fear that the scientific segment of society is by no means free from non-ideal observations, formulations, communications and motivations. However, the experience of mankind gives no hope of a system-wide improvement—we must turn to the individual. A key to the individual response is given in the last phrases of the above quotation from Gibbs' biographer:

> ". . . and no student could come in contact with this serene and impartial mind without feeling profoundly its influence in all his future studies of nature."

We might conjecture that the serenity and impartiality of this mind came from its will and ability to discern in strident

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[5]The word "wealth" as used by classical economists describes the social equivalent of "useful work."

and noisy communications pouring in from all sides the "still small voice" of truth, and its determination to follow rigorously courses of action motivated by this truth and this truth alone. After a century this serene and impartial mind still shines through the great scientific works of Willard Gibbs, giving to the student, overwhelmed by the inconsistencies, the confusion and violence of the world of today, determination to refine the gold of truth from the dross of falsehood and error, and courage to follow this truth relentlessly.

PROFILE

Who is Harry Diamond Labs?

ABSTRACT

A new location for the Harry Diamond Laboratories of the U.S. Army Material Command puts a new emphasis on its research in nuclear weapons effects, advanced radar techniques, fluidics, and instrumentation.

The name is derived from the man, Harry Diamond, born in Russia on February 12, 1900, and emigrated to the United States as a child. A graduate of the Massachusetts Institute of Technology, he received a masters degree in electrical engineering from Lehigh University in 1925. From 1927 until his death in 1948, his career was a long succession of major technical achievements in radio and electronics.

Mr. Diamond joined the staff working

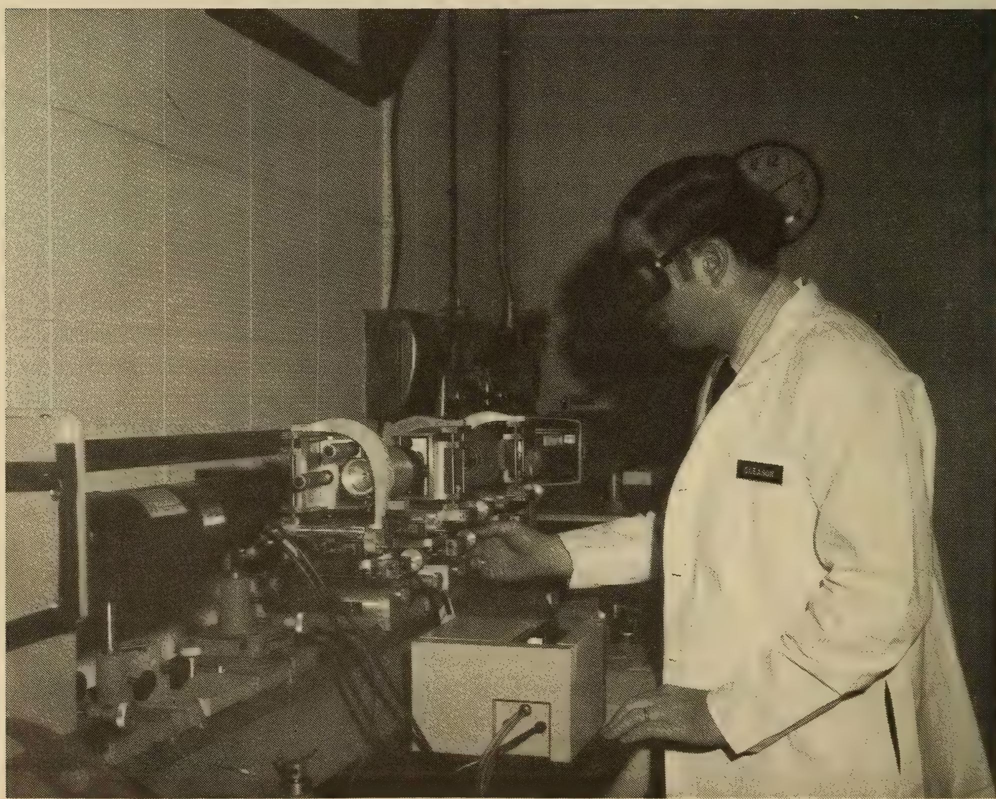

Fig. 1.—Physics research conducted within the lab includes a program to adjust an optical parametric oscillator (turnable laser).

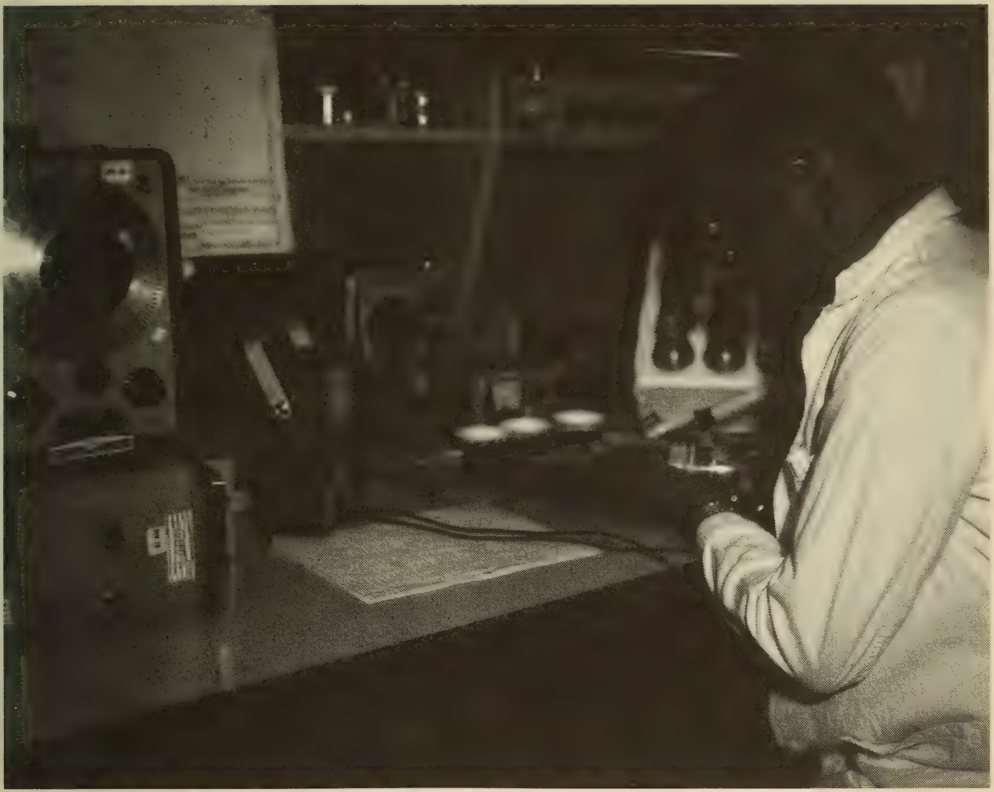

Fig. 2.—Assembling a unique telemetry unit to a mortar round to acquire information during a test firing.

on the first radio-range beacon system for the airways when he came to National Bureau of Standards: a year later he was made chief of the activity. New developments evolved rapidly until the project was transferred out in 1933. The work culminated in the development of the first complete instrumented system for landing aircraft "blind." Today's Instrument Landing System derives from this work of more than 30 years ago.

Next, with his staff, he developed the radiosonde, a balloon-carried automatic weather station; about 2,000,000 have been launched. It is still the primary means of measuring atmospheric conditions which determine weather.

About a year before Pearl Harbor, Harry Diamond was given responsibility for the Bureau's part in developing proximity fuzes for non-rotated missiles. It was calculated—and proved in combat—that a fuze which would explode a warhead near an air-borne target or at the best height above a surface target would increase damage by a factor of five or ten. Much of the basic proximity fuze technology was developed under his direction.

At the end of World War II, Mr. Diamond became interested in still more advanced electronic systems and in new electronic component developments. Printed circuits were given their first real start in this program, and considerable progress was made in automatic assembly process high-polymer potting compounds, and electrical transducers and controls.

While the initial charter of the organization directed all of its efforts into weapons fuzing, its current activities

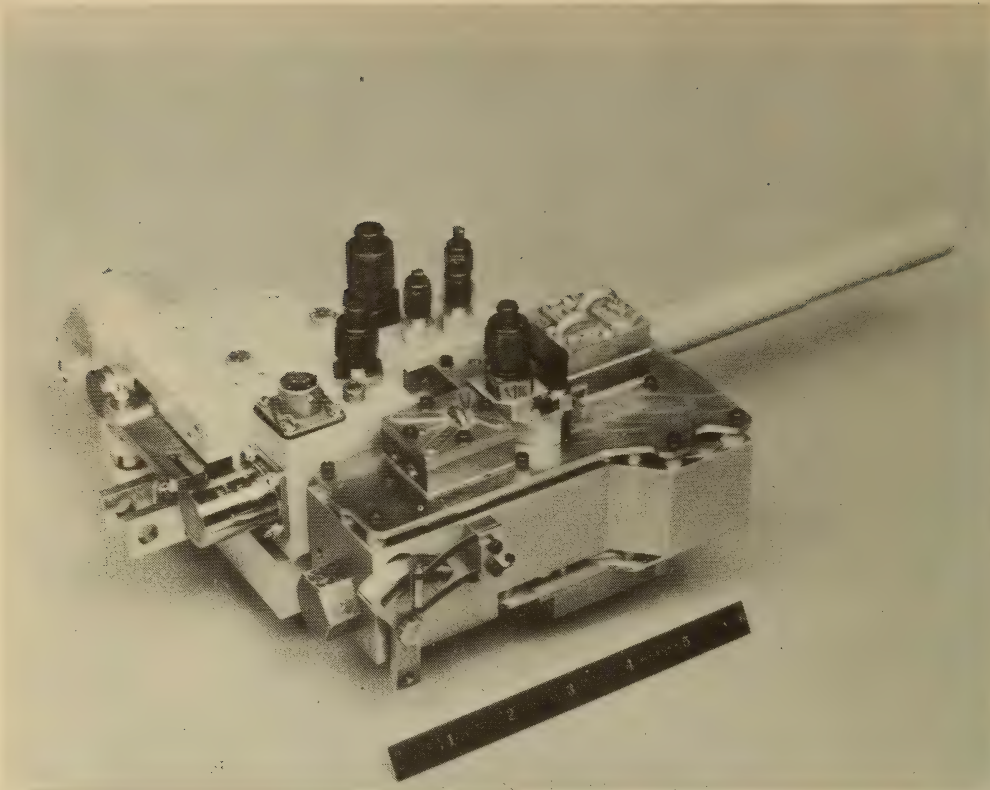

Fig. 3.—In 1960, HDL announced the invention of the first family of fluidic devices that have since evolved into the development of a whole new technology including a three-axis stability augmentation system for helicopters that uses no moving parts.

(including proximity fuzing) range through a wide variety of interests. Among the more important fields covered are:

Nuclear Weapons Effects.—HDL pioneered in the field of transient radiation effects (TREE) in the internal and external electromagnetic pulse (EMP) effects on electronics. Presently, it is the lead laboratory in this area for the Army Materiel Command and co-ordinates all AMC activities in this field. To harden electronic components for tactical and strategic systems, HDL maintains nuclear, X-ray, gamma, and electromagnetic pulse simulators that are used by many elements of DOD.

Advanced Radar Techniques.—Present emphasis is upon advanced-radar techniques for target detection and signature analysis for a wide range of environment. The technique of combining coherent radar signals in phase and in

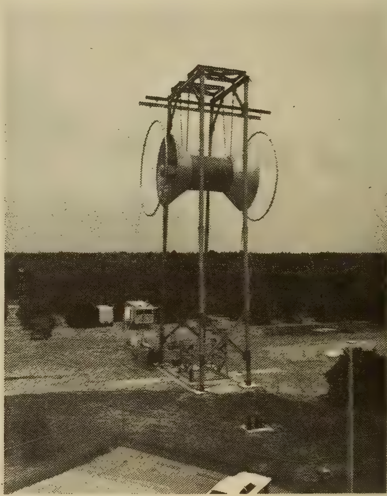

Fig. 4.—The first transportable Electromagnetic Pulse Simulator (EMP) was developed by HDL in 1973. In support of Defense Nuclear Agency and Defense Communication agencies, it is being used to study the effects of EMP on telecommunication systems throughout the U.S.

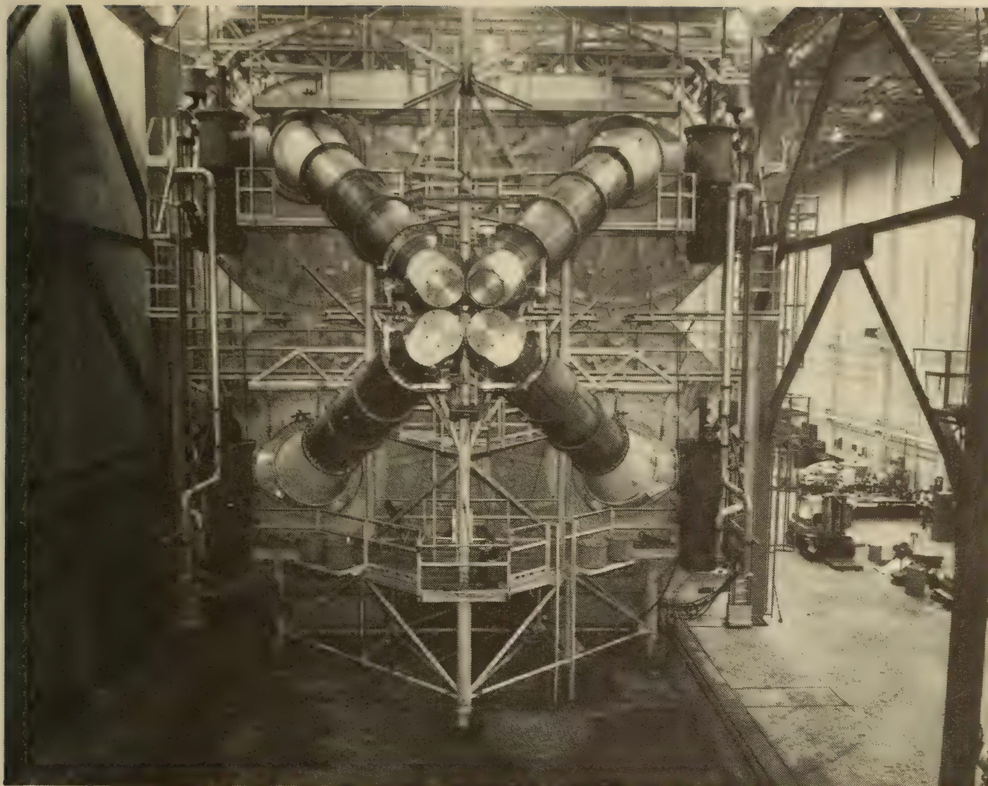

Fig. 5.—The AURORA facility is operated as a tri-service facility "to lessen the dependence of the nation on underground testing of weapon systems." The Defense Nuclear Agency funded the facility to study the ionizing effects of gamma rays and fast neutrons. The output of four Marx generators and four Blumlein pulse forming networks are synchronized to form a radiation pulse 100 billionths of a second long.

quadrature to obtain the direction of the target's movement was conceived and developed by HDL for the purpose of detecting moving targets in a cluttered environment.

Fluidics.—Since the invention of the first family of fluid devices at HDL in 1960, it has been the lead laboratory for AMC on the application of fluidics.

Instrumentation.—The task of recovering information from in-flight artillery shells and missiles has led to a highly refined capability to apply ruggedized, miniaturized electronic devices by HDL. An early recognition of this capability was the 1957 micro-miniaturization award won by HDL for the application of photo-lithographic techniques to transistor production.

The Laboratories have a complement of approximately 1500 personnel led by 500 engineers and scientists, who are principally electronic and mechanical engineers and physicists. Last September, the Laboratories marked its twentieth anniversary by affixing the cornerstone at its new facility in Adelphi, Maryland.

When construction is completed in 1976, the installation will utilize 90 of 137 acres for laboratory and support buildings. The remaining areas will be devoted largely to a buffer zone from the adjacent residential and business area. The site located five miles from the northeast boundary of the District of Columbia, straddles the line between Montgomery and Prince George's Counties in suburban Maryland and was transferred from the Naval Ordnance Laboratory, White

Oak, Md., in late 1969. The collocation of two such recognized R & D activities is anticipated to not only support but reinforce the other in their similar mission assignments while keeping with government objectives of minimizing duplicative programs.

U. S. Army Corps of Engineers and Harry Diamond Laboratories project officers for the planning and construction of the research complex are proud that it is staying close to cost estimates despite the impact of inflation. They also consider it unique in several design engineering aspects, assuring that facilities will be "fully matched to HDL needs."

Located at 2800 Powder Mill Road in Adelphi, the HDL complex is designed in the form of an H. In addition to adjacent support facilities, 600 or more feet away, it consists of four buildings linked together by the administration center at the front end and a center building crossway between General Purpose Laboratories 1 and 2. The crossway provides a secure courtyard at the east end and a west courtyard accessible to visitors.

In January 1974, HDL occupied General Purpose Lab 1 and three support buildings. By January 1975, the Administration wing and the center building crossing will be completed, bringing the number of employees relocated to Adelphi to approximately 900.

Variation and Synonymy in Hypselonotus *(Heteroptera: Coreidae)*

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ABSTRACT

In the coreid genus *Hypselonotus* Hahn, 35 specific and varietal names, most in current use, are available. Analysis of chromatic variation reveals that 4 species are complexly varied, and that these 35 names pertain to 9 species here recognized. The following new synonymies are proposed: *H. bitrianguliger* Berg 1892 (=var. *mendax* Horváth 1913); *H. fulvus* (De Geer) 1773 (=*lanceolatus* Walker 1871 and var. *gentilis* Horváth 1913); *H. interruptus* Hahn 1833 (=*atratus* Distant 1881, *balteatus* Horváth 1892, *andinus* Breddin 1901, var. *hilaris* Horváth 1913, and *aberrans* Horváth 1913); *H. linea* (Fabricius) 1803 (=*proximus* Distant 1881, *loratus* Breddin 1901, *fuscus* Osborn 1904, var. *procerus* Horváth 1913, *pedestris* Horváth 1913, *simulans* Horváth 1913, and *aequatorialis* Horváth 1913); and *H. lineatus* Stål 1862 (=*intermedius* Distant 1881, var. *neglectus* Horváth 1913, and var. *detersus* Horváth 1913). *Hypselonotus lineatus* Stål and *H. punctiventris* Stål are removed from synonymy with *H. fulvus* (De Geer) and reinstated as recognized species.

In order to provide the correct names of Costa Rican species of *Hypselonotus* in ecological studies by L. A. Real and D. H. Janzen, University of Michigan, I found it necessary to do a short study of chromatic variation to determine which of some 35 available names merit recognition and which pertain to phenotypic and geographic variants. The most recent comprehensive treatment of the genus was by Horváth (1913). His treatment is in general clear and easy to interpret, except that some forms were not available to him. I accept his work as reliable, since it is likely that he had access to type material. Despite his careful treatment, however, I think his interpretation of species much too narrow; thus, I treat as synonyms many names considered by Horváth to represent full species. I do not recognize sympatric varietal names; and I do not recognize subspecies since, at least at present, few if any can be adequately defended as geographic entities.

Various names proposed by Walker (1871) were not treated by Horváth, who probably regarded them as not congeneric with *Hypselonotus*; as judged from original descriptions, these excluded names are not conspecific with forms treated here. Other names not treated by Horváth are *linea* Fabricius, *thoracicus* Signoret, and *fuscus* Osborn. Types of these were not examined, but I think they are readily identifiable from original and subsequent descriptions.

In this study, based on material in the United States National Museum of Natural History (USNM), I attempted to

Key to Species of *Hypselonotus*

1.	Femora armed with paired subapical ventral teeth	2
	Femora unarmed	5
2(1).	Femora with two rows of fine denticles before subapical teeth; corium wholly red	*bitrianguliger* Berg
	Femora without distinct denticles before subapical teeth; corium wholly black or black with yellow maculation	3
3(2).	Corium wholly black	*thoracicus* Signoret
	Corium with yellow maculation	4
4(3).	Dorsum black except for bold yellow stripe along midline of pronotum, scutellum, claval suture, and interior margin of corium	*linea* (Fabricius)
	Dorsum black except for yellow external margin on pronotum and elytron	*tricolor* Breddin
5(1).	Femora distinctly annulated with dark spots or bands	6
	Femora unicolorous	*interruptus* Hahn
6(5).	Rostrum with three distal segments black	7
	Rostrum with three distal segments pale, or diffusely infuscated or with one or two black segments in some South American specimens	8
7(6).	Abdomen without spots, or with median row of spots only	*lineatus* Stål
	Abdomen with five rows of spots	*subterpunctatus* Amyot & Serville
8(6).	Abdomen with four or five rows of spots	*punctiventris* Stål
	Abdomen without spots	*fulvus* (De Geer)

determine how geographic samples are related and thus how to treat the available names. My procedure was to organize material geographically and to search for geographically and chromatically intermediate forms. Sympatric chromatic forms are treated as distinct if independently varied, or as phenotypic variants if not independently varied.

Hypselonotus bitrianguliger Berg

Hypselonotus bitrianguliger Berg 1892: 101.
Hypselonotus bitrianguliger var. *mendax* Horváth 1913: 372. *New synonymy.*

This species differs from all others by having the undersurfaces of the femora biserrulate as well as apically bidentate. I regard the 2 named forms as minor color varieties. Horváth (1913) reported specimens of both forms from "Rio Grande do Sul," Brazil. Among material examined, specimens with the pronotal markings weakly developed (*mendax*) are from Villarrica and "São Paulo." This species is known from northern Argentina, southeastern Brazil, and Paraguay.

Material examined.—BRAZIL. Paraná: Rondon (1). Santa Catarina: Nova Teutonia (1). São Paulo: "São Paulo" (4), Sorocaba (1). PARAGUAY. No Locality (3). Guaira: Villarrica (1).

Hypselonotus fulvus (De Geer)

Cimex fulvus De Geer 1773: 341.
Hypselonotus fulvus: Dallas 1852: 464.
Cimex striatulus Fabricius 1775: 721.
Lygaeus striatulus: Fabricius 1794: 161.
Hypselonotus striatulus: Burmeister 1835: 320.
Lygaeus venosus Fabricius 1794: 142.
Hypselonotus venosus: Stål 1868: 56.
Hypselonotus fulvus var. *venosus:* Horváth 1913: 369.
Hypselonotus dimidiatus Hahn 1833: 189.
Hypselonotus striatulus var. *dimidiatus:* Horváth 1913: 369.
Hypselonotus lanceolatus Walker 1871: 140. *New synonymy.*
Hypselonotus lanceolatus var. *gentilis* Horváth 1913: 369. *New synonymy.*

These names are based on minor variations in color of beak, pronotum, and corium. There is some geographic basis to this variation, but the forms are not sufficiently constant to merit recognition as subspecies. Variation is particularly complex in western South America. This species is related to *H. lineatus* and *H. punctiventris*, but all specimens are dis-

tinguished from the former by the pale beak and from the latter by complete lack of abdominal maculation.

The corium in most specimens is banded, but in some from Colombia, Venezuela, Trinidad, Brazil, and Peru it is unbanded (*fulvus, lanceolatus, striatulus*). In most banded specimens the band is triangular, but in some from "Colombia," Venezuela (Caracas, Esmeralda), Guyana (Kartabo), Brazil (Manaus, Tefé), Bolivia (Cavinas), and Peru (Iquitos, Pucallpa, Yurimaguas) it is transverse and narrow. In some specimens from Peru, the last 2 or even 3 segments of the rostrum are diffusely darkened (*lanceolatus, gentilis:* Iquitos, "Peru," Pucallpa, Yurimaguas), and in one from Caracas the second segment is black while the other segments are pale.

The pronotum in *lanceolatus* and *gentilis* is strongly vittate and not nigropunctate, in *fulvus* and *venosus* nigropunctate and not strongly vittate, and in *striatulus* and *dimidiatus* neither nigropunctate nor vittate. The distribution of nigropunctate and non-nigropunctate forms is geographic: generally nigropunctate in Panama, Colombia, Venezuela (except Caracas and Esmeralda), Guyana, Surinam, Trinidad, and northern Brazil ("Amazon," Manaus, Pará, Tefé; also one specimen from Rio de Janeiro); non-nigropunctate elsewhere. This distinction is imperfect, and in some nigropunctate specimens the dark punctations are few. In some specimens from Bolivia and Peru (Iquitos, "Peru," Yurimaguas) the pronotum is strongly vittate, in others (Cavinas, Pacallpa, "Peru") faintly vittate, and in another (Chanchamayo) non-vittate. These all have strongly developed preocellar vittae. Others from the same area have the pronotum non-vittate and lack preocellar vittae ("Bolivia," "Peru," Chanchamayo, Coroico, Tingo Maria). Within this area, specimens with the pronotum faintly to strongly vittate all have a narrow corium band while in the other specimens the band is absent, triangular, or has weakly defined apical limits. Elsewhere, the pronotum is faintly vittate in some specimens with the nigropunctate pronotum (some specimens from Brazil and Guyana) and in one with the pronotum non-nigropunctate (Esmeralda). Preocellar vittae are developed in all nigropunctate specimens, but also in some non-nigropunctate specimens from Brazil (Chapada, Nova Teutonia), Bolivia, and Peru.

McAtee (1919) placed *H. punctiventris* as a synonym of *H. fulvus*, but I think these 2 forms deserve recognition at least at subspecies level. I have seen 1 specimen of *fulvus* from Belize, and 4 of *punctiventris* from Belize and Guatemala, but have seen none of either from elsewhere in Central America north of Panama. The *fulvus* and *punctiventris* specimens do not intergrade: the *fulvus* specimen lacks pronotal vittae and abdominal spots, and is much smaller.

Material examined.—No locality (7). BELIZE. Toledo: San Antonio (1). PANAMA. Canal Zone: Bobio (3), Juan Mina (1), Tabernilla (1). Panamá: Colón (4), Panamá (1). COLOMBIA. No locality (1). Antioquia: Medellín (3). Córdoba: San Jerónimo (1). Cundinamarca: El Colegio (2). Magdalena: Rio Frio (3). Meta: Villavicenzo (3). Tolima: Armero (2). Valle del Cauca: Cali (2), Palmira (2). VENEZUELA. No locality (5). Esmeralda (1). Miranda (1). Aragua: Maracay (1). Distrito Federal: Caracas (4). Monagas: Quiriquire (2). Yaraguy: San Felipe (1, on "*Sida caprifolia*"). TRINIDAD. No locality (6), Caparo (3), Caranege (1), Caroni River (8), D'Abadie (1), La Brea (1), Maracas Valley (3, on "*Cordia cylindrostachia*"), Palo Seco (1), Port-of-Spain (2), River Estate (1), Saint Augustine (5, on *Cordia* and pigeon peas), San Fernando Hill (1). GUYANA. Blairmont Plantation (2). Demerara: Georgetown (9). Essequibo: Bartica (1), Kartabo (10). SURINAM. Marowijne: Moengo (1). BRAZIL. No locality (1). "Amazon" (1). Amazonas: Manaus (4), Tefé (3). Bahia: Caldeiras (1). Ceará: Ceará (1). Mato Grosso: Chapada (3), Corumba (2), Rio Caraguata (1). Minas Gerais: Sabara (1), Vicosa (1). Pará: Pará (1). Pernambuco:

Bonito (4), Recife (1). Rio de Janeiro: Rio de Janeiro (4). Santa Catarina: Nova Teutonia (13). São Paulo: Campinas (2), Guaituba (1), São Paulo (1). PARAGUAY. Central: Asunción (1), Luque (1). Concepción: 45 mi. e. Horqueta (8). Guaira: Villarrica (1). Paraguari: Sapucay (1). ARGENTINA. Jujuy: Calilegua (1). Misiones: Misiones (1), Posada (1). Tucumán: Tucumán (2). CHILE. Colchagua: Cordillera de los Cipreses (1). BOLIVIA. No locality (4). Bení: Cavinas (1). La Paz: Coroico (3). PERU. No locality (3). Chanchamayo (2). Huánuco: Tingo Maria (3). Loreto: Iquitos (3), Pucallpa (1), Yurimaguas (1).

Hypselonotus interruptus Hahn

Hypselonotus interruptus Hahn 1833: 187.
Hypselonotus bilineatus Westwood 1842: 21.
Hypselonotus concinnus Dallas 1852: 465.
Hypselonotus lineaticollis Stål 1855: 185.
Hypselonotus interruptus var. *lineaticollis:* Horváth 1913: 370.
Hypselonotus propinquus Walker 1871: 142.
Hypselonotus concinnus var. *propinquus:* Horváth 1913: 370.
Jadera subvittata Walker 1871: 145.
Hypselonotus subvittatus: Horváth 1913: 370.
Hypselonotus atratus Distant 1881: 152. *New synonymy.*
Hypselonotus balteatus Horváth 1892: 260. *New synonymy.*
Hypselonotus andinus Breddin 1901: 25. *New synonymy.*
Hypselonotus atratus var. *hilaris* Horváth 1913: 370. *New synonymy.*
Hypselonotus aberrans Horváth 1913: 370. *New synonymy.*

I found no morphological features to distinguish any of these forms, and think they are best treated as 1 geographically varied species. If this treatment is correct, variation is more complex than in other species of the genus. Particularly in South America, 2 or more phenotypes may exist in the same or in nearly proximate localities, with no or incomplete intergradation. However, sufficient intergradation exists to justify treatment of all these forms as conspecific. The following discussion of variation is arranged by geographic area, from north to south.

Specimens from Mexico are *concinnus* or *propinquus*, or intermediates. No geographic differentiation is evident. In all specimens, the head lacks extensive dark maculation on jugum or ocellar tubercles, the pronotum lacks paired white vittae, the scutellum is pale, and the abdomen is unspotted. In *propinquus* the corium is wholly pale and the pronotum is pale except for 2 small basal spots, while in *concinnus* the corium is banded and the pronotum is more extensively darkened but with the midline pale. *Material examined.*—MEXICO. No locality (13). Chiapas (2). Colima: Colima (4). Distrito Federal: Mexico (3), Tacubaya (2). Guerrero: Ixcuinatoyac (1), Rincon (1), Xucumanatlán (1). Morelos: Cuernavaca (4), Hujintlán (2), Tepoztlán (1). Oaxaca: Isthmus of Tehuantepec (1), 44 mi. e. Juchitán (2), Oaxaca (1). Tabasco: Teapa (1). Veracruz: Atoyac (1), Córdoba (6), Jalapa (4), Orizaba (1).

Specimens from Belize, Guatemala, and Honduras are *concinnus*, with the following exceptions. Some specimens from Acatenango and Yepocapa are nearly as pale as in *propinquus*. One of 10 specimens from Punta Gorda is *andinus* (extensive black on jugum and ocellar tubercles, scutellum vittate, pale band of corium narrow, abdomen spotted) and another is an *andinus-concinnus* intermediate (abdomen not spotted). No other Central American specimens have the abdomen spotted. One of the 5 specimens from Chiquimula is a pale *balteatus* (pronotal maculation reduced, with paired white lines on each side; in this specimen the corium is wholly pale and the scutellum dark), and 3 are *atratus* (scutellum black; pronotal collar black at least in part). The *balteatus* specimen is the only pure *balteatus* from Central America. One of 16 specimens from Morales is *atratus*. One of 2 specimens from Trece Aguas is an *atratus-concinnus* intermediate, with scutellum partially darkened and with small black spots on the pronotal collar. *Material examined.*—BELIZE. Uyace Peak (1). Belize: Belize (1). Toledo: Punta Gorda (10). GUATEMALA. No locality (8). Bananera (1). Cayuga (1).

Finca Los Cerritos (1). Alta Verapaz: Trece Aguas (2). Chimaltenango: Acatenango (4), Yepocapa (71). Chiquimula: Chiquimula (5), El Naranjo (3, on *Cinchona*). Guatemala: Guatemala (1). Izabal: Livingston (1), Morales (16), Puerto Barrios (1). Sacatepequez: Antigua (5). Sololá: Olas de Moka (1). HONDURAS. Francisco Morazán: Tegucigalpa (2), Zamorano (1).

One specimen from northwestern Costa Rica (Palo Verde) is *concinnus*. All other specimens from Costa Rica and western Panama are either pure *atratus* or the minor variety *hilaris* (pronotal collar pale). In some, the dark areas of the pronotum and scutellum are divided by a fine pale line visible only under magnification. This form is known also from Guatemala where it intergrades with *concinnus*, and from central Panama where it intergrades with both *concinnus* and *balteatus*. *Material examined.*—COSTA RICA. Guacimo (1). Navarro Farm (2). Cartago: 3 mi. w Turrialba (1), Volcan Irazú (2). Guanacaste: Bagaces (Palo Verde) (1), Pozo Azul (3). Puntarenas: Monteverde (1). San José: Candelarita (1), Escazú (6), San Carlos (3), San Jose (11), Santiago Puriscal (2). PANAMA. Chiriquí: Boquete (13).

Specimens from central and eastern Panama are mostly pure *concinnus* or intermediates, but some pure *atratus* and *balteatus* are represented. Some specimens from Colombia are *concinnus* or *balteatus-concinnus*, some are *balteatus*, and one (Rio Dagua) is *andinus-concinnus*. All specimens from Venezuela are *balteatus*. In series of *balteatus* from Rio Frio and Los Teques, the corium is banded in some specimens and wholly pale in others. In some specimens of various phenotypes from Colombia and Venezuela the pale band of the corium is narrow. One specimen from Trinidad is *balteatus-concinnus*. *Material examined.*—PANAMA. No locality (1). Canal Zone: Ancon (3), Barro Colorado (1), Bobio (1), Cabima (21), Corazal (1), Las Cruces (1), Limon (3), Pedregal (1), Summit (7). Darién: Sabanas (1). Panamá: Taboga Island (5). COLOMBIA. No locality (1). Antioquia: Medellín (8). Caldas: Chinchina (2). Cundinamarca: Bogotá (4), El Colegio (1). Magdalena: Rio Frio (11). Meta: Rio Meta (1). Narino: Pasto (1). Santander: Values Cararé (2), Landazuri (3). Valle del Cauca: Cali (3), Rio Dagua (1). VENEZUELA. Amazonas: Culebra (1). Aragua: Rancho Grande (1). Miranda: Los Teques (14, on "*Coffea arabica*"). TRINIDAD. Port-of-Spain (1).

Specimens from Bolivia (Christal-Mayu), Brazil, Paraguay, and Argentina have a distinct white annulation at the base of antennal article three, and have the corium banded (*interruptus*), wholly pale (*lineaticollis*), or intermediate. These specimens otherwise have the characteristics of *balteatus*. In northern Brazil ("Amazon"), specimens are obviously intermediate in that the antennal annulation is narrowed. *Material examined.*—BOLIVIA. No locality (2). Cochabamba: Christal-Mayu (1). BRAZIL. No locality (5). "Amazon" (2). Alto de Sera (1). Distrito Federal: Jacaré pagua (1). Mato Grosso: Campo Grande (1), Chapada (5), Ouro Preto (4), Rio Caraguata (1). Minas Gerais: Vicosa (1). Paraná: Curitiba (1). Pernambuco: Bonito (1). Rio de Janeiro: Bico do Papagaio (1), Nova Friburgo (3), Rio de Janeiro (4), Teresopolis (5). Rio Grande do Sul: Porto Alegre (1). Santa Catarina: Nova Teutonia (38). São Paulo: Campinas (3), Guaituba (5), Maua (2), São Paulo (6). PARAGUAY. No locality (1). Guaira: Villarrica (6). Paraguari: Sapucay (25). ARGENTINA. Puesta (1). Misiones: Ignacio (1). Salta: Salta (1). Tucumán: Tucumán (1).

In specimens from Ecuador, Peru, and Bolivia (except Christal-Mayu) there are 3 types of abdominal maculation: 7 rows of spots (*aberrans*: "Bolivia", 1; Carabaya, 1; Ivon, 1; Ixiamus, 1; Rio Blanca, 7; Rurrenabaque, 1; Santa Isabel, 3; Tumupasa, 4); 2 or 4 rows of spots (*andinus*: Cachabi, 7; Chimbo, 1; Pallatango, 2; Quevedo, 3; Rio Pescado, 7; Santa Rosa, 1); and no spots (unnamed: "Bolivia," 4; Ixiamas, 3; Rio Chapare, 1; Rurrenabaque, 3; Tumupasa, 17). In

specimens from Ecuador and Peru (Rio Chapare) the pale band of the corium is narrower than in specimens from Bolivia, indicating relationship with Colombian specimens. In most specimens with the abdomen spotted the pronotum is extensively maculated and has dark lateral margins, but the pronotal maculation is reduced in some Ecuadorian specimens with abdominal spots (Chimbo, Pallatango, Quevedo, Rio Pescado, Santa Rosa) and is strongly developed in some specimens without abdominal spots (Rio Chapare, some Bolivian specimens). The scutellum is wholly black in some Bolivian specimens with abdomen unspotted but is vittate in all others. There is no significant variation in maculation in series from any of the Ecuador localities. In series from three Bolivia localities, however, spotted and unspotted specimens are represented in each. I regard these color forms as conspecific because in both forms the first segment of the beak has a more strongly developed white annulation than in more northern specimens and because some specimens of each agree in pronotal and scutellar coloration. I also regard them as conspecific with *interruptus* because some specimens have paired white pronotal vittae as in *balteatus* and *interruptus*, because specimens with similar dorsal maculation are known from Colombia and northward, and because some Bolivian specimens have a trace of the antennal annulation characteristic of *interruptus*. *Material examined.*— ECUADOR. Cachabi (7). Rio Blanca (7). Chimborazo: Pallatango (2). El Oro: Santa Rosa (1). Guayas: Chimbo (1). Los Rios: Quevedo (3). Manabi: Rio Pescado (7). PERU. Rio Chapare (1). Cuzco: Santa Isabel (3). Puno: Carabaya (1). BOLIVIA. No locality (5). Bení: Ivon (1), Rurrenabaque (4), Tumupasa (21). La Paz: Ixiamas (4).

In summary, the color variants of *H. interruptus* are essentially geographic phenotypes, with the exceptions of a pale *balteatus* in Guatemala (normally in Colombia and Venezuela) and a Chimbo-like *andinus* in Belize (normally in Ecuador and Peru). Pale and dark forms are sympatric in Mexico (*concinnus*), Colombia and Venezuela (*balteatus*), and Brazil and Paraguay (*interruptus*). Intergrades are known for all geographic variants, but in some areas 2 or more phenotypes may occur with little or no intergradation. Thus, in specimens from Bolivia the abdomen is either conspicuously spotted or is unspotted. The pattern of variation is least understood in western South America; further study is particularly needed in Bolivia, where *interruptus* and *aberrans* occur in adjacent departments with little intergradation.

Hypselonotus linea (Fabricius)

Lygaeus linea Fabricius 1803: 220.
Hypselonotus linea Dallas: 1852: 465.
Hypselonotus proximus Distant 1881: 153. *New synonymy.*
Hypselonotus loratus Breddin 1901: 25. *New synonymy.*
Hypselonotus fuscus Osborn 1904: 199. *New synonymy.*
Hypselonotus loratus var. *procerus* Harváth 1913: 370. *New synonymy.*
Hypselonotus pedestris Horváth 1913: 370. *New synonymy.*
Hypselonotus simulans Horváth 1913: 371. *New synonymy.*
Hypselonotus aequatorialis Horváth 1913: 371. *New synonymy.*

These names pertain to geographic color varieties. There is a complex pattern of variation, too complex to fully resolve here and too complex to permit recognition of subspecies. Horváth (1913) did not recognize *linea* in his material and was not familiar with *fuscus*, but these names are readily recognized from the literature (Distant 1881, Osborn 1904, Horváth 1913).

All specimens from Volcan de Chiriquí and northward (*proximus*) are distinguished from all specimens from central Panama and southward by having the elytral fascia parallel to the corium margin rather than bent away from the lateroapical angle. All specimens from Costa Rica and northward have the upper thoracic pleural spots discrete, while in some specimens from Volcan de Chiriquí and in all specimens from central Panama

and Ecuador these spots are fused. The jugum is partly darkened in some Central American specimens, and preocellar spots are present in some.

No specimens are represented in USNM material between Panama and Ecuador, but no important differences between Central and South American specimens were found. Further, specimens from Panama and Ecuador agree in the form of markings of the thoracic pleura.

Horváth named *aequatorialis* and *simulans* for Ecuadorian and Peruvian specimens with univittate pronota and dilated elytral fasciae. Specimens from Paramba and Cachabi have annulate femora, lack preocellar vittae or jugal markings, and have the upper spots of the thoracic pleura fused (*aequatorialis*). Specimens from Hacienda Maria have dark femora, jugum dark laterally, and discrete pleural spots (*simulans*). Two other specimens have broad elytral fasciae but have trivittate pronota: 1 from Venturia is otherwise as in *aequatorialis*, while 1 from Iquitos has notably wide elytral fasciae and otherwise agrees with *simulans* except for having annulate femora. From remarks by Horváth (1913), I suspect the characteristics of the Iquitos specimen are close to or the same as those of the type of *linea*.

Two forms, *simulans* and *pedestris*, have dark femora. These Peruvian color variants were both reported by Horváth from Marcapata. In *pedestris* the elytral fasciae are not dilated (this difference is slight), the jugum is more extensively though not completely darkened (variable), and the pronotum is trivittate. I examined seven specimens of *pedestris* from Calanga, Rio Chapare, and Tingo Maria.

Horváth distinguished *loratus* and *procerus* from *pedestris* chiefly by the annulate femora, and Osborn's *fuscus* fits here by locality and description. Some but not all specimens have dark preocellar markings, and in most specimens the jugum is wholly dark. This form ranges widely in Bolivia, Brazil, and Peru. The variety *procerus* was distinguished by having quinquevittate rather than trivittate pronota and by having dark rather than pale corium venation. Both pronotal variants are represented in series from Chanchamayo, Ixiamas, and Satipo. Forms with pale venation are known only from Bolivia and Brazil, but in series from Huachi and Ixiamas some specimens are fully dark. In Peru, *procerus* and *pedestris* were both reported by Horváth from Pozuzo.

Material examined.—BELIZE. Toledo: Punta Gorda (1), San Antonio (1). GUATEMALA. Chiquimula: Chiquimula (2). COSTA RICA. "Waldeck" (2, on "*Sida rhombifolia*"). Cartago: Carillo (3), Turrialba (5). Guanacaste: Pozo Azul (6). Limón: Guapiles (4), Parismina (3). San José: San Carlos (1). PANAMA. Canal Zone: Alhajuelo (1), Cabima (1), Rio Trinidad (5). Chiriquí: Volcan de Chiriquí (7). Panamá: Cerro Campana (2), El Valle (5). ECUADOR. Cachabi (2), Paramba (5), Venturia (1). PERU. Chanchamayo (15), Chapare (1). Cuzco: Calanga (3), Hacienda Maria (3). Huánuco: Tingo Maria (2). Junín: Satipo (4). Loreto: Iquitos (1). Pasco: Oxapampa (1). San Martín: Tarapoto (1). BOLIVIA. Bení: Huachi (5), Ivon (1), Rosario (1), Rurrenabaque (13), Tumupasa (7). La Paz: Ixiamas (13). BRAZIL. "Amazon" (1). Minas Gerais: Vicosa (1).

Hypselonotus lineatus Stål

Hypselonotus lineatus Stål 1862: 297.
Hypselonotus intermedius Distant 1881: 151. *New synonymy.*
Hypselonotus lineatus var. *neglectus* Horváth 1913: 369. *New synonymy.*
Hypselonotus lineatus var. *detersus* Horváth 1913: 369. *New synonymy.*
Hypselonotus fulvus: McAtee 1919: 9.

These forms were distinguished for variants in color pattern. They cannot be distinguished consistently, and the geographic pattern is too complex to merit recognition of subspecies. I examined 132 specimens from Mexico, Central America, and northwestern South America. These may be grouped into 5 geographic areas (Fig. 1):

Area 1.—Pacific slope of Mexico

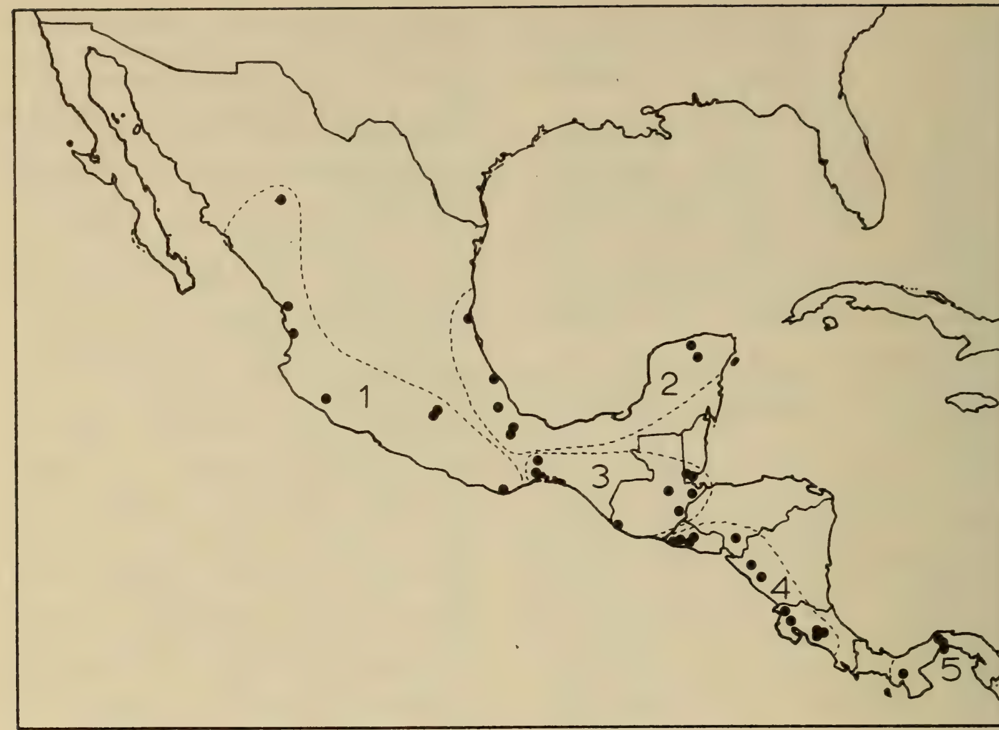

Fig. 1. Distribution and variation of *Hypselonotus lineatus* in Mexico and Central America. See text for description and discussion of color variants in the 5 geographic areas indicated by broken lines.

north of the Isthmus of Tehuantepec. All specimens have six moderate to strong longitudinal pronotal vittae, short preocellar vittae, wide preapical band on corium, and small median spot on one or more of abdominal sterna 3-5. *Material examined.*—MEXICO. Colima: Colima (6). Durango: Presidio (1). Morelos: Cuernavaca (1), Hujintlán (1), Puente de Ixtla (1). Nayarit: San Blas (1). Oaxaca: Puerto Angel (1). Sinaloa: La Concha (1).

Area 2.—Atlantic slope of Mexico. All specimens have 6 strong longitudinal pronotal vittae, long preocellar vittae, and no abdominal spots. In about half of the specimens the preapical band of the corium is narrow, in the other half it is absent. This is true *lineatus*. *Material examined.*—MEXICO. "Mexico" (9). Oaxaca: Tuxtepec (1), Valle Nacional (2). Tamaulipas: Tampico (1). Veracruz: Córdoba (4), San Rafael-Jicaltepec (4). Yucatán: "Yucatán" (1), Chichén Itzá (1), Temax (2).

Area 3.—Isthmus of Tehuantepec to Guatemala. Variation in this area is complex. Some specimens from the Isthmus of Tehuantepec and Belize have pronotal vittae. Some specimens from throughout the area have long preocellar vittae, and/or abdominal spots, and/or wide preapical corium band. These variations are correlated neither with one another nor with sex. The type of *intermedius* is from this area (San Geronimo, Baja Verapaz, Guatemala), and has abdominal spots but lacks pronotal vittae. *Material examined.*—MEXICO. Oaxaca: Almoloya (12), "Isthmus of Tehuantepec" (1), Tolosa (1). BELIZE. Toledo: Punta Gorda (5), San Antonio (2). GUATEMALA. Alta Verapaz: Seganguin (1). Chiquimula: Chiquimula (5). Izabal: Morales (2). Retalhuleu: Champerico (2).

Area 4.—El Salvador to Costa Rica. All specimens lack pronotal vittae and none have long preocellar vittae; elytral and abdominal characters are as in area 3. A series from Piedras Negras (type

locality of *detersus*) includes specimens with the characters of *detersus, neglectus,* and *intermedius. Material examined.* —"Volcan Isalco" (1). EL SALVADOR. "El Salvador" (1). Cuscatlán: Rosario (2). La Libertad: San Andres (2). San Salvador: San Salvador (2). Sonsonate: Acajutla (1). HONDURAS. Francisco Morazán: Zamorano (2). NICARAGUA. León: Guadalupana (2). Managua: Managua (1). COSTA RICA. Salinas (1). Alajuela: Orotina (1). Guanacaste: Bagaces (Palo Verde) (5), Santa Rosa (1). San José: Candelarita (2), Piedras Negras (9), San Carlos (1), Santa Ana (1).

Area 5.—Panama and northwestern South America. All specimens lack long preocellar vittae, pronotal vittae, and abdominal spots; eight specimens from Panama have the wide preapical corium band. *Material examined.*—PANAMA. Canal Zone: Barro Colorado Island (2), Cabima (3), Comacho (1), Juan Mina (6), Paraiso (2), Summit (2), Upper Rio Indio (1). Panamá: Panamá (2), Taboga Island (2). Veraguas: Santiago (1). COLOMBIA. Valle del Cauca: Rio Dagua (3). ECUADOR. El Salado (1). PERU. Tequetepeque (2).

Hypselonotus punctiventris Stål

Hypselonotus punctiventris Stål 1862: 297.
Hypselonotus fulvus: McAtee 1919: 9.

Van Duzee (1917) listed 2 species of *Hypselonotus* from the United States: *H. punctiventris* Stål and *H. fulvus* var. *venosus* (Fabricius), the latter a doubtful record from Texas. McAtee (1919) suggested that all names for Nearctic members of the genus are synonymous with *H. fulvus* (De Geer). However, in USNM material there are 3 specimens of true *punctiventris* from Texas and Mexico variously labelled by McAtee as *H. fulvus, H. fulvus* var. *lineatus,* and *H. fulvus* var. *punctiventris*. I conclude that the only Nearctic species is *H. punctiventris*. It is possible that *punctiventris* is conspecific with *fulvus*, but if so it should be treated as a well marked geographic subspecies; *punctiventris* ranges south through Mexico to Belize and Guatemala, where it overlaps but does not intergrade with *fulvus*.

As implied by the lack of names in synonymy, there is little conspicuous variation. The pronotum tends to have the 6 longitudinal vittae less developed in most specimens from Texas and northeastern Mexico than elsewhere. In all specimens preocellar vittae are present, and in all specimens the pale band of the corium is present and triangular.

Material examined.—UNITED STATES. Arizona: Santa Cruz County, near Nogales (1). Texas: No locality (2); Bee County, Beeville (2); Bexar County, San Antonio (8); Cameron County, Brownsville (17, on "*Abutilon hypoleucum*," cotton, and "*Wiesidula holosericea*"); Dimmit County, Asherton (1, on blackeyed pea), Carrizo Springs (3, on blackeyed pea and eggplant); Duval County, San Diego (1); Guadalupe County, Seguin (1); Hidalgo County, Thayer (2, on ragweed); Jim Wells County, Alice (2); Nueces County (4); Victoria County, Victoria (20, on cotton, *Croton*, and flowers). MEXICO. No locality (15). Coahuila: Zaragoza (1). Colima: Colima (8). Distrito Federal (1). Durango: Ventanas (1). Jalisco: Chapala (1), Sayula (1). Morelos: Cuautla (2), Cuernavaca (21, on "*Eupatorium adenophorum*"). Nuevo Leon: Linares (1). Oaxaca: Almoloya (1). Salina Cruz (1), San Geronimo (11), Tehuantepec (1), Tlacolula (1). San Luis Potosí: Tamazunchale (2). Sinaloa: Villa Union (1). Tamaulipas: Ciudad Victoria (4), Matamoros (9, on "*Pseudabutilon lozani*"), Tampico (1). Veracruz: Córdoba (1), Pueblo Viejo (3). Yucatán: Temax (2). BELIZE. Toledo: Punta Gorda (2). GUATEMALA. Chiquimula: Chiquimula (2).

Hypselonotus subterpunctatus Amyot & Serville

Hypselonotus subterpunctatus Amyot & Serville 1843: 242.

I cannot judge the true status of this form. The black beak suggests affinity with *H. lineatus*, but the spotted abdo-

men is distinctive and no intergrades are known. All specimens examined have a wide transverse pale band on the corium.

Material examined—BOLIVIA. Cochabamba: Christal-Mayu (10). La Paz: Ixiamas (1). BRAZIL. Mato Grosso: Rio Caraguata (3). São Paulo: Campinas (1).

Hypselonotus thoracicus Signoret

Hypselonotus thoracicus Signoret 1862: 581.

This is the only described species with wholly black elytra, and was described from Yurimaguas, Peru. I have seen nothing to exactly match the original description. I have seen 1 specimen from Peru with wholly black elytra, but the head is red rather than yellow and the pronotum and abdomen are differently maculated. This specimen is similar also to *H. tricolor* but has the head largely rufous rather than testaceous, pronotum trivittate and with black margins, elytra wholly black, and abdomen densely maculated.

Material examined.—PERU. San Martín: Tarapoto (1).

Hypselonotus tricolor Breddin

Hypselonotus tricolor Breddin 1901: 25.

Specimens of this species are readily distinguished by having the pronotum and elytra black with pale margins. This species was reported by Horváth (1913) from 2 localities in Peru.

Material examined.—No locality (1).

Discussion

Hypselonotus is represented in Mexico and Central America by 5 abundant, widespread species, 3 of which are also abundant and widespread in South America. Of these, *H. fulvus, H. lineatus,* and *H. punctiventris* are closely related. *Hypselonotus lineatus* and *H. punctiventris* are sympatric throughout Mexico and are independently variable, and thus are unquestionably reproductively isolated. *Hypselonotus fulvus* is principally South American but extends through northwestern South America and Central America in sympatry with *H. lineatus* and marginally overlaps *H. punctiventris* in Guatemala. Only 1 specimen of *H. fulvus* was examined from north of Panama and, though I think it unlikely, *H. fulvus* may intergrade with *H. punctiventris*. It is even less likely, but not impossible, that *H. fulvus* and *H. lineatus* may intergrade in western South America, where some specimens of *H. fulvus* have the rostrum darkened and the pale band of the corium narrow.

Four of these widespread species are extremely varied geographically, notably in Central America and western South America. Southern Mexico and Guatemala form the principal area of intergradation of various forms of *H. lineatus*, but even as far south as Costa Rica samples of this species are not uniform. Clinal variation in maculation of pleura and elytra occurs in *H. linea* from Costa Rica to Ecuador, and more complex chromatic variation occurs from Ecuador to Bolivia. Complex chromatic variation, only partly geographic, occurs throughout the range of *H. interruptus* but is most extreme in Central America and from Ecuador to Bolivia. Similar complex chromatic variation occurs in *H. fulvus* in the area between Ecuador and Bolivia. Careful field studies of populations and genetics are needed in these areas.

Four localized South American species are less well known. *Hypselonotus bitrianguliger* is morphologically distinctive, but its color forms need further study. *Hypselonotus thoracicus* and *H. tricolor* are probably distinct from *H. linea* but may not be distinct from each other. *Hypselonotus subterpunctatus* probably is most closely related to *H. lineatus* but probably is not conspecific with that species since no intergrades are known; it is sympatric with *H. fulvus* but differs chromatically and is independently varied.

Acknowledgements

I thank my friends R. C. Froeschner, R. D. Gordon, J. L. Herring, D. H. Janzen, J. M. Kingsolver, and L. A. Real for assistance and

comment. Funding was provided by D. H. Janzen from NSF Grant 35032X.

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A New Species of Zonosemata *Benjamin from Colombia (Diptera: Tephritidae)*

George C. Steyskal

Systematic Entomology Laboratory, IIBIII, Agr. Res. Serv., USDA. Mail address: % U. S. National Museum, Washington, D. C. 20560.

ABSTRACT

Zonosemata ica, new species, is described from Colombia. It is the first South American representative of the genus, otherwise known only from North America and the Antilles.

The genus *Zonosemata* Benjamin consists of 5 species found in North America (Bush, 1966; Foote, 1967). The southernmost records so far are of *Z. vidrapennis* Bush from the State of Oaxaca, Mexico and *Z. minuta* Bush from Jamaica, almost as far south. Two specimens of a 6th species have been received from Dr. Lazaro Posada O., chief of the entomological section of the Instituto Colombiano Agropecuario. These are the 1st records of the genus from South America. The known hosts of the North American species are plants of the family Solanaceae (fruits of horse nettles and eggplant, *Solanum* spp., and of peppers, *Capsicum* spp.). Unfortunately no host of the new species herein described is known.

Zonosemata ica Steyskal, new species

Female.—Length of wing 4.5 mm. Very similar to *Z. vittigera* (Coquillett), agreeing with that species in the 1st paragraph of the key by Bush (1965: 313) in having distinct black spots in the presutural area of the mesoscutum, although lacking a black spot on the sternopleuron. Differences from the described species of *Zonosemata* are chiefly in the color pattern of the body and wing.

Color yellowish, with white J-shaped supra-alar stripe, humerus, and medial stripe (ending broadly rounded before scutellar suture and extending very narrowly to anterior margin of thorax), and with dark-brown to black marks as follows: crescentic spot at anterior end of yellow stripe mesad of humerus, small squarish patch about base of anterior notopleural bristle, broad band in scutellar suture faintly connected at each end with pair of diffuse spots on each pair of submedian yellow stripes behind midway of distance from scutellar to transverse suture, spot on postalar declivity near base of wing, hourglass-shaped spot on pteropleuron between pteropleural and sternopleural bristles, small spot at lower base of scutellum, metanotum (including postscutellum) in full width of scutellum, small spot dorsal to base of halter, oval spot on last preabdominal tergum at width of spot from lateral margin of tergum, small dot in similar position on penultimate tergum, narrow apical margin of ovipositor sheath and more extensive bases of sclerotic strips in base of ovipositubus.

Wing with brown pattern similar to that of *Z. vittigera* (Bush, 1966: 309, f. 5), with rather broad and dark stripes from humeral vein to extension of anal cell, from pterostigma through anterior crossvein to wing margin, and from costal to hind margin through posterior crossvein. Anterior end of latter stripe, however, faint in cells R_1 and R_3 and brownish costal margin to apex of wing also faint; bar between latter 2 stripes in cells R_1 and R_3 similarly faint and closer to stripe through posterior crossvein than to one through anterior crossvein; these latter transverse stripes faintly connected near posterior margin of wing.

Cheek 0.52 as high as width of face between vibrissal angles (in *Z. vittigera* 0.67 to 0.81 as high).

Ovipositor 1.25 mm long by 0.18 mm wide, parallel sided but rapidly tapering to simply aciculate point.

Holotype, female, Cúcuta, Colombia, January, 1974 (L. Nuñez), ICA no. 727; paratype (sex?, apical half of abdomen lacking), Blonay, Colombia, June, 1973 (J. A. Martinez), ICA no. 63, with right wing on microscope slide; both specimens captured in traps in coffee trees; type no. 73063 in U. S. National Museum.

The species name is a noun taken from the acronym ICA of the Instituto Colombiano Agropecuario. I am indeed grateful

to the authorities of that Instituto for the privilege of studying these flies.

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SIX SCIENTISTS RECEIVE ACADEMY'S ANNUAL AWARDS

Awards for outstanding scientific achievement were conferred upon three research scientists and three science teachers at the Annual Awards Dinner meeting of the Academy on March 21, 1974.

The research investigators honored were Dr. Ronald B. Herberman of the National Cancer Institute, in the biological sciences; Mr. Thomas N. Pyke, Jr. of the National Bureau of Standards, in the Engineering Sciences; and Dr. Jogesh Chandra Pati, of the University of Maryland, in the Physical Sciences.

The science teachers honored were Dr. Joseph A. Bellanti, School of Medicine, Georgetown University; Dr. Phillip I. Connors, Physics Department, University of Maryland; and Mr. Robert Leroy Wistort, High Point Senior High School, Beltsville, Maryland.

Biological Sciences

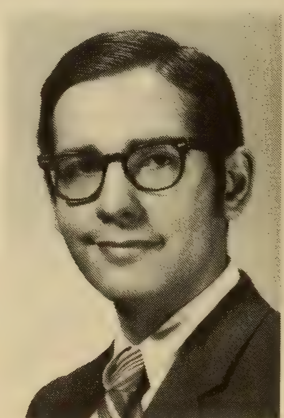
Ronald B. Herberman was cited for "his scientific contributions in developing an important area of cancer immunology, for selective leadership in fostering, coordinating, and encouraging research in cellular immune reactions to human tumor-associated antigens."

Dr. Herberman was born December 26, 1940 in Brooklyn, New York. He received his BA in 1960, *summa cum laude*, from New York University and his M.D. in 1964 from New York University School of Medicine. His internship and first year residency in medicine were spent at Massachusetts General Hospital 1964-66. From 1966-68, he was a clinical associate in the Immunology Branch of the National Cancer Institute, moving up to Senior Investigator in 1968 and to Head of the Cellular and Tumor Immunology Section, Laboratory of Cell Biology in 1971, his present position.

Ronald B. Herberman

Engineering Sciences

Thomas N. Pyke, Jr. was cited for "valuable contributions to the field of



Thomas N. Pyke, Jr.

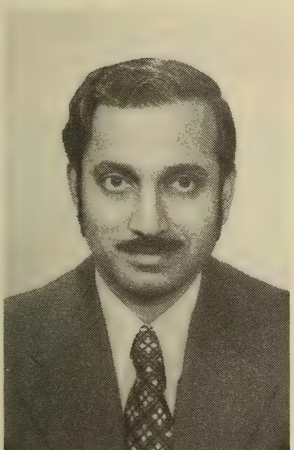
computer networking technology."

Mr. Pyke was born July 16, 1942 in Washington, D.C. He obtained his B.S.E.E. in 1964 from Carnegie Mellon University, and his M.S.E. in 1965 from the University of Pennsylvania. He was a student trainee at the National Bureau of Standards from 1960-64, Chief of the Computer System Section ICST, 1969-73. In 1973, he became Acting Chief of the Computer Systems Engineering Division, and Chief of the Computer Networking Section for Sciences and Computer Technology, the position he now holds.

Physical Sciences

Dr. Jogesh Chandra Pati was cited "for contributions toward a unified theory of elementary particles."

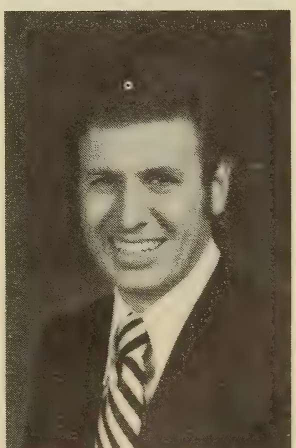
Dr. Pati was born on April 3, 1937. He received his B.S. with Honors from Ravenshaw College, Utkal University, India, in 1955, his M.S. in 1957 from Delhi University, and his Ph.D. in 1961 from the University of Maryland. He held a Tolman Postdoctoral Fellowship at California Institute of Technology (1960-62). He was a member of the Institute for Advanced Study at Princeton (1962-63) and a visiting Scientist at the Brookhaven National Laboratory during the summer of 1963. He is presently in the Department of Physics and Astronomy at the University of Maryland. During the past year and in cooperation with Dr. Abdus Salam, Director, International Center for Theoretical Physics, Trieste, Italy, he has proposed a unified gauge theory of the strong, electromagnetic and weak interactions. A successful unified theory is the dream of all particle physicists, and this is one of the most promising which has been put forward in the context of the new gauge approach to elementary particle interactions.



Jogesh C. Pati

Teaching of Science

Medical School.—Joseph A. Bellanti, Professor of Pediatrics, School of Medicine, Georgetown University, was cited for "being an outstanding scientific investigator and dedicated teacher." He is the youngest faculty person ever to be elevated to full professorship in the School of Medicine at Georgetown University. He was named as one of the "Outstanding Educators of America in 1972," a listing recommended by College Presidents and Deans. He has authored a textbook entitled, "Immunology" published by Saunders, 1971. Currently this book is being used as a textbook in over 50 medical schools



Joseph A. Bellanti

throughout the country. The textbook has been widely acclaimed for its success in presenting the difficult principles of immunology in a straightforward manner, easily comprehended by students.

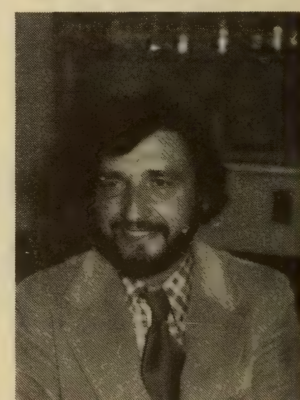
Dr. Bellanti was born in Buffalo, New York, November 21, 1934. He received his B.A. from the University of Buffalo, (Phi Beta Kappa) in 1954 and his M.D. from the University of Buffalo in 1958. He joined the Georgetown University School of Medicine in 1963 as an Assistant Professor of Pediatrics and Microbiology, rapidly advancing up the ranks to Associate Professor in 1967 and to full professor in 1970, his present position.

University.—Phillip I. Connors, an assistant Professor in Physics at the University of Maryland, was cited for being an effective and innovative teacher who has been particularly successful in elementary courses for nonscience majors. He pioneered in the use of self-paced or Keller Plan teaching methods and developed the first entirely self-paced course at the University. He has also played a unique and important role in the improvement of college science teaching in general in the Washington and Chesapeake Bay Area by his administration and direction of three important programs: As Director, since 1969 of The Chesapeake Physics Association, aimed primarily at improving the scientific competence and level of teaching at small colleges; as Director of COSIP, College Science Improvement Program Maryland Physics Consortium, aimed primarily at the Community Colleges; and as Director of NSF Summer Institutes in Physics offered to strengthen the background of teachers at predominantly black colleges.

Phillip I. Connors

Dr. Connors was born October 7, 1937 in Norfolk, Virginia. He obtained his B.S. in 1959 from the University of Notre Dame, his M.S. in 1962, and his Ph.D. in 1966, both from Pennsylvania State University. He was an instructor at Pennsylvania State University in 1963, a Junior Research Associate at Brookhaven National Laboratory 1963-65, a Research Associate at the University of Maryland 1965-69, becoming an Assistant Professor in 1969, his current position.

High School.—Robert Leroy Wistort, a biology teacher at High Point Senior High School, in Beltsville, Maryland, was cited for being a truly outstanding teacher, exceptionally talented in his ability and accomplishments in inspiring so many young people to love and enjoy science. He holds to the concept that students kept under high pressure will



Robert L. Wistort

produce to the best of their ability and he gets results. He arouses the curiosity and interest of his students in timely biological problem-solving situations and he has developed a curriculum for the course and contributed to a county curriculum for the advanced biology classes. Several years ago he was recognized as the "Outstanding Biology Teacher" in the State of Maryland. He contributed to the development of the BSCS curriculum (Ecology version) in Boulder, Colorado, and he has taught at National Science Foundation Institutes held at Howard University.

Mr. Wistort was born on January 5, 1929 in Chicago, Illinois. He received his B.S. from the University of Illinois, and had almost completed his Masters degree in Zoology before the Army intervened. He is currently completing another Master's degree in Human Development at the University of Maryland.

BOARD OF MANAGERS MEETING NOTES

Feb. 7, 1974

The 624th meeting was called to order at 8:00 p.m. by President Sherlin in the conference room in the Lee Building at FASEB.

Secretary.— Dr. Stern moved and Dr. Noyes seconded that the minutes be approved as corrected. Passed.

Treasurer.— Dr. Rupp presented the 1973 annual report. He noted that income from dues, mutual funds and the Journal sales was less than expected while Journal costs were higher. Total deficit was about $5000. The Executive Committee met and discussed the 1974 budget. To balance the budget a dues increase is necessary. Dr. Rupp moved and Dr. Abraham seconded that the dues be increased by $3.00 and the Journal by $2.00 as in the proposed budget. Passed. Dr. Stern suggested that we appeal for voluntary contributions. Mr. Sherlin suggested that each member of the Board of Managers bring in five new members. Dr. Stern moved and Dr. Robbins seconded that the Journal subscription rate be raised to $12.00 domestic and $13.00 foreign. Passed. President Sherlin will appoint a committee to study the office expenses in relation to the Philosophical Society and JBSEE. Dr. Forziati moved and Dr. Boek seconded that the budget be accepted. Passed.

Membership.— Mrs. Forziati presented seven nominees for fellowship. They are: Frank J. Adrian, Jay P. Boris, Joseph M. Botbol, Charles M. Guttman, Elizabeth O'Hern, and Barry N. Taylor. Two new delegates were also presented: George E. Hudson, Philosophical Society, and Robert F. Cozzens, Chemical Society. Dr. Boek moved and Dr. Stern seconded that these candidates for fellowship be accepted. Passed.

A letter from Dr. Mebs on a recent trip reported that he had seen Rev. Francis J. Heyden, S.J. who is in the Philippines. His address is: Astronomical Observatory, P.O. Box 1231, Manila, Philippines D404. A letter will be drafted to Rev. Heyden encouraging his scientific studies in the Philippines.

Grants-in-Aid.— Dr. Shropshire said they had made 16 awards for $466. Forty-three applications were received. A simple report will be asked from each student. Mr. Sherlin indicated that we had unspent funds for three projects. There is a committee for each project.

Program.—Dr. Abraham suggested that we start an annual lecture series called the Gibbs lecture. In February the 543rd meeting will be the "Centennial of Gibbs' Thermodynamics." The speakers will be: Dr. Raymond Mountain, who will speak on "A Geometrical Description of Critical Phenomena;" Douglas Rumble III will speak on "Gibbs' Phase Rule and its Application to Geochemistry;" and Harold J. Morowitz will speak on "A Biologist's View of Gibbs' Contributions." Dr. Abraham moved and Dr. Robbins seconded that we have an annual Gibbs lecture during the year. Passed. Questions of any finances will be referred to the Finance Committee, as no commitments were made for funds.

A lecture series on Solar Energy starting March 14 will be jointly sponsored by the Institute of Electrical and Electronics Engineers and WAS at the Univ. of Md. There were no commitments made for support by WAS. In the future other joint lecture series might be cosponsored and possibly be funded with grants.

Symposium.—Dr. Forziati said that the grant was signed for publication costs. He did not know the number enrolled for the symposium.

Science Achievement.—Dr. Aldridge reported the awardees are: Biological Sciences, Dr. Ronald Herberman, NIH, for his scientific contributions in developing an important area of cancer immunology; Engineering Sciences, Mr. Thomas N. Pyke, Jr., NBS, for valuable contributions to the field of computer networking technology; Physical Sciences, Dr. Jogesh Chandri Pati, Univ. of Md., for contributions towards a unified theory of elementary particles; Teaching of Science, Medical Science, Dr. Joseph A. Bellanti, Georgetown Univ., Dr. Phillip I. Connors, Univ. of Md., Mr. Robert L. Wistort, High Point Senior High School, Beltsville, Md. It was moved by Dr. Forziati, seconded by Dr. Robbins that the Board go on record that the awards dinner be held in April in 1975. Passed. Dr. Aldridge moved that these candidates be approved, seconded by Dr. DePue. Passed. (See elsewhere this issue. Ed.)

Membership Promotion.—Dr. O'Hern stated that one membership notice had appeared in the Journal. Affiliated societies are asked to nominate members. Seventy-five new members have joined between June and February.

Joint Board on Science and Engineering Education.—February is to be established as the time for the annual payment to JBSEE.

Management Policy.—A committee will be appointed to make their report at the March meeting.

Election Results.—President Sherlin read the new officers for 1974-75.

President: Dr. Kurt Stern
President-elect: Dr. George Abraham
Secretary: Dr. Mary Aldridge
Treasurer: Dr. Nelson Rupp
Managers-at-Large: Dr. William Bickley, Mr. Richard Farrow.

The next Board meeting will be held in the first two weeks of March. Dr. Weissler moved, seconded by Dr. Sulzbacher, that the meeting be adjourned. Passed.

The meeting adjourned at 9:45 p.m.—Patricia Sarvella, *Secretary*.

Mar. 12, 1974

The 625th meeting was called to order at 8:10 p.m. by President Sherlin in the conference room in the Lee Building at FASEB.

Secretary.—George E. Hudson asked to have his middle initial included in his name in the minutes. Dr. Robbins moved and Dr. Rowen seconded that the minutes be accepted as corrected. Passed.

Treasurer.—No report. A letter was sent to members requesting contributions.

Science Achievement.—Dr. Aldridge reported that everything is ready for the banquet and that a report has been prepared for the Journal. Elizabeth Ostaggi will take care of the publicity for the annual awards dinner.

Membership.—Mrs. Forziati reported on two candidates for fellowship. The citation on Richard Donovick will be changed to conform with the format of the other fellows. Dr. Rupp moved that the candidates be elected to fellowship (Victor E. Adler, Richard Donovick). Seconded by Dr. Robbins. Passed. Miss Ostaggi will notify the candidates of their election.

Symposium on Statistics and the Environment.—Dr. A. Forziati reported there were about 150 people present. It appears that expenses were met. Many attendees wanted a sequel. He raised the question whether we wanted to participate but not publish the proceedings. Publicity about the Symposium was not sent out to industry since expenses limited the mailing distribution. The size of the cafeteria limited the size of the Symposium. Mr. Garber recorded the sessions. The tapes are being transcribed as fast as run off.

Dr. Forziati suggested that we have another symposium next year, possibly on "Energy-Generating Impact on the Environment." Topic ideas should be given to Dr. Stern.

Joint Board on Science and Engineering Education.—Montgomery County will have a Science Fair this year with the National Bureau of Standards as a co-sponsor so they don't have to obtain insurance. Copies of press releases were passed out. The Science booklet is provided by NBS, the affiliation fee by JBSEE, and the student expense to the International Fair by Montgomery Area Science Fair Association.

Program Committee.—Dr. Abraham reported on three meetings coming up this year: in Sept. a joint meeting with the Jr. Academy and on Nov. 21 the Gibbs Lecture. Perhaps meetings with other societies should be continued, since they were well attended this year.

Science Fair.—Dr. Stern said that we should encourage the member societies to go around to the Area Science Fairs and give certificates, etc. An article might be written up and put in the Journal.

Journal.—Dr. Foote reported that the printer will soon be back on schedule with the Journal. The June issue will be on the Symposium. Only two acceptable manuscripts have been received so far. The grant from EPA is $2500, or 25% of the money spent, whichever is less. The bookkeeping will have to be very carefully done. By the end of March there should be an idea of the length of the Symposium issue. The size will be kept to a minimum.

Fellows.—Mr. Sherlin again requested that each member of the Board of Directors should try to obtain five new fellows. The society representatives should try to compare the list of their members with the fellows. New fellows will be introduced at the annual meeting. There will be a meeting in April to process new fellows.

AAAS.—In 1977 the AAAS will meet in Washington.

Dr. Rupp moved that the meeting be adjourned, seconded by Dr. Noyes. Passed. The meeting adjourned at 9:30 p.m.—Patricia Sarvella, *Secretary*.

Paul R. Achenbach, Chief, Building Environment Div., Ctr. for Bldg. Tech., NBS, in recognition of his contribution in Mechanical and Electrical Engineering and in particular for his many years of successful research management of building environment investigations. Additional notice is taken of his contributions to engineering standards and to recent programs on energy conservation. *Sponsors:* Grover C. Sherlin, Max Tryon, John W. Rowen.

Victor E. Adler, Research Entomologist, USDA, for his contributions to insect olfaction, and in particular his researches on the electrophysiological responses of insects to attractants and pheromones. *Sponsors:* Martin Jacobson, Morton Beroza, Milton S. Schechter.

Edith R. Corliss (Mrs.), Physicist, Sound Section, NBS, in recognition of her contributions to acoustics, and in particular for her researches on the physical measurement of the parameters of speech and hearing. *Sponsors:* Richard K. Cook, Martin Greenspan, Daniel P. Johnson.

Richard Donovick, Director, American Type Culture Collection, Rockville, Md., in recognition of his contributions to scientific literature in the areas of fermentation and antibiotic research. He is now director of American Type Culture Collection. *Sponsors:* R. R. Colwell, H. Finley.

Donald F. Flick, Health Sciences Coordinator, Food & Drug Adm., in recognition of his contributions to nutrition and biochemistry, particularly his researches on the metabolic effects of chlorinated hydrocarbons. *Sponsors:* Carleton R. Treadwell, Mary Louise Robbins, H. W. Mandel.

Allan L. Forsythe, Director of Special Projects & Science Dept. Chairman, St. Albans Sch., Washington, D.C., in recognition of his outstanding teaching of science to young men and women in their formative years. Especially notable is the integration of several disciplines in his course in aquatic systems. *Sponsors:* Jean K. Boek, Bernard B. Watson, George W. Irving.

William B. Fox, Head, Inorganic Chemistry Branch, Chemistry Div., Naval Res. Lab., in recognition of his contributions to the field of inorganic fluorine chemistry, particularly his research on the synthesis of high-energy species, the characterization of unstable intermediates at cryogenic temperatures, and the elucidation of the structures and chemistry of hypervalent molecules. *Sponsors:* Kurt H. Stern, Fred E. Saalfeld, David R. Flinn.

Robert N. Goldberg, Chemist, NBS, in recognition of his contributions to chemical thermodynamics, and in particular his research on the application of irreversible thermodynamics to electrochemical cells having liquid junctions. *Sponsors:* John W. Rowen, Donald D. Waxman, Kelson B. Morris.

Louis S. Jaffe, Assoc. Clinical Professor of Epidemiology & Environmental Health, George Washington University, in recognition of his contributions to environmental medicine, in particular his valuable surveys of the toxicological implications of carbon monoxide, ozone, and other atmospheric pollutants for man and other biological systems. *Sponsors:* J. Murray Mitchell, Jr., Donald H. Pack.

Eugene Jarosewich, Chemist-supervisory, Smithsonian Institution, in recognition of his demonstrated exceptional skill in analytical chemistry of complex substances such as meteorites, minerals, lunar samples and rocks. *Sponsors:* E. P. Henderson, A. Wetmore.

Berenice G. Lamberton, retired (Lecturer, Georgetown University, 1962–

72), in recognition of her contributions to science education and in particular her exceptional span of service, extending over 26 years from 1947 to date, in the science encouragement programs of the WAS. *Sponsors:* Grover C. Sherlin, Max Tryon, John W. Rowen.

Julius Lieblein, Mathematical Statistician, NBS, in recognition of his contributions to the statistical theory of extreme values and their application to failure phenomena. *Sponsors:* John W. Rowen, Churchill Eisenhart, Grover C. Sherlin.

Henry S. Liers, Research Physicist in Special Studies Office, Naval Research Laboratory, in recognition of his contributions to nuclear physics and in particular his researches on nuclear polarization phenomena. *Sponsors:* George Abraham, A. Schindler, Leland A. DePue.

Thomas P. Meloy, Vice President, Research & Development, Meloy Labs, Springfield, Va., in recognition of his contribution to mineral engineering, in particular his theoretical contributions to the particle science field. *Sponsors:* George Abraham, Grover C. Sherlin, William Gage.

Raymond D. Mountain, Chief, Statistical Physics Section, NBS, in recognition of his contributions to statistical mechanics, with particular applications to the study of the equilibrium and transport properties of liquids. *Sponsors:* Grover C. Sherlin, Nelson W. Rupp, Max Tryon.

James H. Mulligan, Jr., Secretary/Executive Officer, National Academy of Engineering, in recognition of his contributions to network theory and electronic feedback systems. *Sponsors:* George Abraham, A. Schindler.

Hajime Ota, Agricultural Engineer, USDA, in recognition of his contributions to agricultural engineering, especially in the areas of poultry housing and environmental conditions. *Sponsors:* Patricia Sarvella, Grover C. Sherlin, Robert E. Menzer.

Anton Peterlin, Physicist, NBS, in recognition of his contribution to rheology, rheoptics and light scattering of dilute polymer solutions and to the study of the relationship between physical properties and morphology of crystalline polymer solids. *Sponsors:* John W. Rowen, Grover C. Sherlin, James M. Cassel.

Jenny E. Rosenthal, Physicist, Dept. of the Army, in recognition of her achievement in spectroscopy, optics, and mathematical techniques and their application to electronic engineering. *Sponsors:* George Abraham, Lowell Ballard, Richard K. Cook.

Marjorie R. Townsend, Project Manager, Small Astronomy Satellite, Goddard Space Flight Center, in recognition of her contributions to electrical engineering and in particular her accomplishments as project manager of the Small Astronomy Satellites. Additional recognition is given to her service to science education, particularly her work as a member of the JBSEE. *Sponsors:* Grover C. Sherlin, Nelson W. Rupp, Max Tryon.

SPECIAL NOTICE

To: The Members and Fellows of the Academy

From: Nelson W. Rupp, Treasurer

The Academy's financial status has been carefully reviewed and the Board of Managers has voted to increase the annual dues effective on January 1, 1975. The increase to both Fellows and Members is a total of $3.00; two of the dollars are a result of increasing the Journal subscription and the other is added to the dues. This will make the dues as of January 1, 1975, for Fellows $18.00 and Members $13.00.

The increase is necessary to cover the rise in costs for supplies, services and office management. The previous dues increase, voted in 1970, was adequate for the first two years but not for 1973 and 1974. The major contribution to the 1973 deficit was the markedly reduced income from our mutual funds. The Board is seeking means to improve the investment of the reserve funds.

The increase in dues will generate sufficient income to meet 1975's anticipated expenses. Meanwhile, the deficit will continue for calendar year 1974. The Board of Managers, therefore, is asking all Fellows and Members for a contribution to relieve the pressure on this year's budget. The suggested contribution is $5.00 for Fellows and $2.50 for Members. The contribution can be made by check payable to the Washington Academy of Sciences, and can be mailed to the above address.

In addition to the search for improving the income from investments the Board has taken measures to reduce expenses, for example, moving the office into a smaller space, thus reducing the rent. Cost consciousness continues to be a major concern.

For those who have not paid their 1974 (or 1975-Ed.) dues, you may add your contribution and send a check for the total to the Academy.

SCIENTISTS IN THE NEWS

Contributions in this section of your Journal are earnestly solicited. They should be typed double-spaced and sent to the Editor three months preceding the issue for which they are intended.

DEPARTMENT OF DEFENSE

Donald B. Dinger has been installed for a 1-year term as president of the Belvoir Chapter of Sigma Xi/The Scientific Research Society of North America.

He received a BS in electrical engineering from the University of Rhode Island in 1958 and his MS in engineering from George Washington University in 1964. While at URI, he was Outstanding Junior Engineering ROTC Student and Outstanding Senior Electrical Engineering ROTC Student. Since 1958 he has been associated with the U. S. Army Mobility Equipment Research and Development Center at Fort Belvoir, Va., including 6 months active duty there as an Army Corps of Engineers officer. In 1966, he was elected a Fellow of the Washington Academy of Sciences on the basis of his achievement at the Center, where he is currently Associate Technical Director for Research and Development.

GEORGE WASHINGTON UNIVERSITY

Ariel C. Hollinshead, Professor of Medicine of The George Washington University Medical Center, was selected as outstanding cancer scientist by the Board of Directors of AAAS, and was invited by "Znaniye" (knowledge), the 2-½ million member scientific organization of the USSR, to be their guest for the promotion of scientific friendship, good will, and the exchange of information in the field of oncology. Dr. Hollinshead enjoyed both formal and informal scientific exchanges and a very impressive cultural program, which included attendance at the opera both in Moscow and Leningrad, the Red Army Chorus, the Moscow circus, the ballet, and the other first-class performances as well as the special privilege of invitations to the homes of some of the scientists. She was impressed with the warmth, friendliness and expertise of her colleagues in the USSR and by the well organized and extensive influence of the Znaniye groups. While there, she was privileged to visit members of the Soviet Women's Committee of the USSR and to learn of their work in aiding underdeveloped nations and in developing friendship with women professionals all over the world.

Dr. Hollinshead lectured at the Gamaleya Institute of Epidemiology & Microbiology, of the Academy of Medical Sciences, USSR; at the Herzen State Oncologic Institute situated in Moscow; at the Institute of Experimental and Clinical Oncology, Academy of Medical Sciences, USSR; at the Petrov Research Institute of Oncology in Leningrad; at the Pavlov Medical Institute of Leningrad; and other places.

NATIONAL INSTITUTES OF HEALTH

Dean Burk, who has been with the National Cancer Institute since 1939, retired recently after 45 years of Federal service. Dr. Burk headed NCI's Cytochemistry Section, Division of Cancer Biology and Diagnosis, from 1946 until his retirement.

Dr. Burk began his Federal career in 1929 as an associate physical chemist in the Department of Agriculture before joining NCI as a senior chemist.

He is noted for his research on the role of fermentation and the Pasteur reaction in relation to cancer cell growth; studies of the one quantum mechanism and energy cycle in photosynthesis, and more recently, studies on "healthier cigarettes" and cancer chemotherapy.

Dr. Burk is best known for the "Lineweaver-Burk Plot" for the determination of enzyme dissociation constants which

was published in 1934, and for his co-discovery of biotin.

Dr. Burk received both his B.S. and Ph.D. degrees from the University of California. From 1927 until 1929 he was a Fellow with the National Research Council and the International Education Board at the University of London, the Kaiser Wilhelm Institute for Biology, and Harvard University.

His scientific honors include the Hillebrand Award of the American Chemical Society and the Gerhard Domazk Award for Cancer Research. In 1971 he received the National Health Federation Humanitarian Award.

Dr. Burk was made a foreign scientific member of the Max Planck Society in 1953, and was knighted in 1970 by the Medical Order of Bethlehem, founded by the Vatican.

Dr. Burk is the author of 227 papers, and is on the editorial board of the *Record of Chemical Process* and *Enzymologia*.

He will continue to write and lecture, and will serve as a visiting scientist at the National Naval Medical Center. Dr. Burk also plans to go on with his work in portrait painting and his interest in music.

RICE UNIVERSITY

Frederick D. Rossini, Professor of Chemistry, was awarded the degree of Doctor of Philosophy, *Honoris Causa*, by the University of Lund, Lund, Sweden, at exercises held there on May 31, 1974. The award was made in recognition of his scientific work in thermodynamics and thermochemistry, physical chemistry of petroleum and hydrocarbons, and numerical data for science and technology. Rossini was born in Monongahela, Pennsylvania, did his undergraduate work at the Carnegie Institute of Technology (now Carnegie-Mellon University), completed his graduate studies at the University of California-Berkeley, and served on the staffs of the National Bureau of Standards (1928–50), the Carnegie Institute of Technology (1950–60), and the University of Notre Dame (1960–71), before going to Rice University.

OTHER SCIENTISTS

Milton Harris received the Wilbur Lucius Cross Medal of Yale University on May 20. The citation read "dedicated alumnus, chemist, inventor, research administrator, and statesman of science."

R. N. Ghose was elected Chairman of the Board of American Nucleonics Corporation. He had been President of the Company for the last eleven years. Dr. Ghose is a Fellow of the IEEE, IEE (London), Institute of Physics (London), American Association for the Advancement of Science, Washington Academy of Sciences, and APS.

Eugene Weber, Washington, D. C. consulting engineer and Commissioner U. S. Section, International Joint Commission 1948–1973, has received the Can-Am Civil Engineering Amity Award from the Americal Society of Civil Engineers. The Can-Am Civil Engineering Amity Award gives recognition to those civil engineers who have made outstanding and unusual contributions toward the advancement of professional relationships between American and Canadian civil engineers.

Born in Stacyville, Iowa, in 1910, Mr. Weber received his degree in civil engineering at the University of Minnesota in 1930. His civilian service with the Corps of Engineers began in 1931 in the Great Lakes area and New England and concluded with his retirement in 1965 as Deputy Director of Civil Works for Policy in the Office of the Chief of Engineers. For his military assignments during World War II, he received numerous decorations including the Legion of Merit, the Bronze Star Medal, the Army Commendation Medal and French Croix de Guerre. His civilian awards include the Rockefeller Public Service Award, the Army's Exceptional Civilian Service Award and the Defence Department's Distinguished Civilian Service Award.

His activities with the International Joint Commission included the major projects for the mutual benefit of the two

countries. Also, the St. Lawrence Project was one of his principal concerns, as well as the related developments in controlling the levels and flows of the Great Lakes and the preservation of Niagara Falls.

In recent years, the IJC has been concerned with air and water pollution along the entire boundary, but particularly along the Great Lakes, where as a result of an IJC report, President Nixon and Prime Minister Trudeau signed a Great Lakes Pollution Agreement on April 15, 1972. This agreement has given great impetus towards restoring the quality of these boundary waters.

OBITUARIES

Lewis J. Clark

Lewis Jesse Clark, 62, an organic chemist who retired last year as research chemist in the Naval Research Laboratory's metallurgy division, died in George Washington University Hospital following heart surgery. He lived on Newark Street NW.

Dr. Clark, a District resident since 1930, worked for the federal government for about 40 years. A native of Chester, N. H., he received his undergraduate degree from George Washington University in 1937 while working for the U. S. Geological Survey. In 1954 he received his Ph.D. from the University of Maryland, where he studied soil chemistry.

Dr. Clark had a number of papers published in analytical chemistry publications and at the time of his death was writing a paper on titanium determination in iron-base materials.

While at the National Bureau of Standards from 1942 to 1953 Dr. Clark was awarded a medal and certificate for his work on the atomic bomb and a certificate from the Office of Scientific Research and Development.

Later in the 1950s he was a chemist for the Agriculture Department's soil and water conservation research branch and in 1956 became a chemist for the Naval Research Laboratory.

Dr. Clark was a fellow of the American Institute of Chemists and the American Association for the Advancement of Science. He was a charter member of the Geochemical Society. He was active in American Youth Hostels, Audubon Naturalist Society and organizations of All Souls Unitarian Church.

He leaves his wife, Julie C., three sisters Mrs. Warren Butman of Springfield, Mass., Mrs. Francis Mills of Warrenton, Va., and Mrs. Arnold Tayler of Branford, Conn., and four brothers, Charles, of Lebanon, Mo., Fred and Frank, of Manassas, and Lyman, who lives in Kensington.

David Livingston Crawford

1889–1974

David Livingston Crawford, entomologist, author, administrator and educator, died in Moorestown, New Jersey, January 16, 1974 of mesenteric thrombosis. He had been troubled with Parkinson's disease for several years before his death, but in spite of this debilitating disease he remained remarkably active until October 1973, and his mind was alert and clear to the end.

This account of his life and activities records a few of the accomplishments of his distinguished career and provides a list of his taxonomic papers in entomology. The bibliography of 5 titles on the Thysanoptera and 32 on the Psyllidae (Homoptera) will aid librarians, bibliographers, thrips and psyllid workers.

Dr. Crawford's earliest work in insect taxonomy was with thrips, or Thysa-

David Livingston Crawford

noptera, on which he published in 1909 and 1910. His interest in this group seems to have been short lived, however, because he did not continue their study. Simultaneously with his work on thrips, he began the study of the jumping plant lice or Psyllidae, and his interest centered around this group from 1910 to 1928 when he published his first and last taxonomic papers on the family. During this time he acquired many psyllids and in 1945 generously presented his collection to the Smithsonian Institution, Washington, D. C. Most of his thrips collection, however, is in the Canadian National Collection, Ottawa, Canada.

Dr. Crawford's first articles on psyllids appeared while he was an undergraduate student at Pomona College. His comprehensive study, "A monograph of the jumping plant-lice of the New World," published in 1914 by the U. S. National Museum, established him as an authority on this family of insects, and thereafter he received and described numerous psyllids from many parts of the world. Although he maintained an interest in entomology over the years, he had little time to devote to psyllids after becoming president of the University of Hawaii and was unable to properly care for the insects in his private collection. Thus when his collection was received by the Smithsonian, some specimens had been lost and others were fragmented. Still others, however, were in reasonably good condition for delicate, pinned insects that had been shipped long distances. Examples of all species described by Dr. Crawford were not represented in his collection and are presumed to have been deposited in the collections of the institutions which furnished them. Reprints of Crawford papers on the Thysanoptera and Psyllidae are not available from the Smithsonian Institution or the U. S. Department of Agriculture.

David Crawford was born March 7, 1889 in Hermosillo, Mexico, the son of Matthew and Harriet Crawford, church missionaries in Mexico, and the grandson of Albert A. and Susan Thomas Sturges, the first missionaries in Ponape, Micronesia. This heritage doubtless had a strong influence on David as he grew up and considered his life's work. Although he did not enter the religious missionary field, he pioneered in education and good works and in so doing continued, in a sense, the work of his ancestors.

David's early schooling was acquired at private and public schools in or near Claremont, California. His higher education was obtained at Pomona College where he received a B.A. degree in 1911, at Stanford University which awarded him a M.A. degree in 1912, and at Cornell University where he spent one year (1912–1913) as a graduate student. He was awarded the LL.D. degree by Pomona College in 1934 and by the University of Hawaii in 1966.

After instructing at Cornell while a graduate student, working for some time as an entomologist in Mexico, and teaching botany at Pomona for three years, Dr. Crawford in 1917 moved to the 10-year-old College of Hawaii. There he became active in community as well as scholastic affairs. He engaged in welfare and juvenile employment work with the Hawaiian Pineapple Company. He zealously promoted athletic events, coaching champion football teams at the College and officiating in local games and sports. At the College, where was a professor of entomology, he was instrumental in de-

veloping the Extension Department and was its director from 1921 to 1926. During this period the College of Hawaii became the University of Hawaii. Dr. Crawford became its president in 1927, and served in this capacity until 1942. During his 25 years tenure at the University, Dr. Crawford gave providential guidance to the young institution. During these years, he became an authority on the Islands and authored books on them and their people.

Following his retirement from the University of Hawaii and return to the continental United States, Dr. Crawford served the Federal Government as administrator of the War Production Board, the Foreign Economic Administration and as a representative on the Education Committee to study Latin America. He later became a consultant to Pineapple Canneries, Mexico, for three years. In 1948 he became president of Doane College, Crete, Nebraska, from which he retired in 1954. For several years after retirement from Doane, Dr. Crawford lived in Arlington, Virginia and later moved to Moorestown, New Jersey.

During his later years Dr. Crawford and his wife spent a great deal of time researching a vast amount of literature, letters and notes on religious missionary work in the South Pacific, including that carried out by his grandparents. The endeavors of Dr. and Mrs. Crawford culminated in an interesting book, coauthored by them and entitled "Missionary Adventures in the South Pacific" 1967 (Charles E. Tuttle Company, publishers). This volume will be a lasting memorial to this widely experienced, accomplished couple.

Dr. Crawford is survived by his widow, Leona, a daughter, a son, 7 grandchildren, and a brother.

I am greatly indebted to Mrs. Crawford for information on the life of her husband.

D. L. Crawford Publications on Thysanoptera

1909a. Some new Thysanoptera from southern California. I. Pomona J. Entomol. 1: 100–108, illus.

1909b. Some Thysanoptera of Mexico and the South. I. *Ibid.*, 109–119, illus.

1909c. Notes on California Thysanoptera I. *Ibid.*, 120–121.

1910a. Thysanoptera of Southern California. II. *Ibid.*, 2: 149–152, illus.

1910b. Thysanoptera of Mexico and the South. II. *Ibid.*, 2: 153–170, illus.

D. L. Crawford Publications on Psyllidae

1910a. American Psyllidae I (Triozinae). Pomona J. Entomol. 2: 228–237, illus.

1910b. American Psyllidae II (Triozinae). *Ibid.*, 347–362, illus.

1911a. American Psyllidae III (Triozinae). *Ibid.*, 3: 422–453, illus.

1911b. American Psyllidae IV (A partial revision of subfamilies). *Ibid.*, 3: 480–503, illus.

1911c. American Psyllidae V. *Ibid.*, 3: 628–632, illus.

1912a. A note on certain Psyllidae. *Ibid.*, 4: 684.

1912b. A new insect pest (*Trioza alacris* Flor). Bull. Calif. State Comm. Hort. 1: 86–87.

1912c. Indian Psyllidae. Rec. Indian Mus. 7 (pt. 5): 419–435, illus.

1913. New genera and species of Psyllidae from the Philippine Islands. Philippine J. Sci. 8 (sec. D): 293–301, illus.

1914a. A recently described psyllid from East Africa (Hemip.). Entomol. News 25: 62–65, illus.

1914b. A Monograph of the Jumping Plant-Lice or Psyllidae of the New World. U. S. Nat. Mus. Bull. 85: i–ix + 186, illus.

1915. Ceylonese and Philippine Psyllidae (Homoptera). Philippine J. Sci. 10 (sec. D): 257–269, illus.

1917. Philippine and Asiatic Psyllidae. *Ibid.*, 12 (sec. D): 163–175, illus.

1918. The jumping plant lice (family Psyllidae) of the Hawaiian Islands. Proc. Hawaii. Entomol. Soc. 3: 430–457, illus.

1919. The jumping plant lice of the Palaeotropics and the South Pacific Islands—Family Psyllidae, or Chermidae, Homoptera. Philippine J. Sci. 15: 139–207, illus.

1920a. New or interesting Psyllidae of the Pacific Coast (Homop.). Entomol. News 31: 12–14.

1920b. Notes on Psyllidae (Homoptera). *Ibid.*, 31: 69–70.

1920c. Cerotrioza (Psyllidae, Homoptera). Proc. Hawaii. Entomol. Soc. 4: 374–375, illus.

1920d. The Psyllidae of Borneo. Philippine J. Sci. 17: 353–361, illus.

1924a. New Indian Psyllidae. Rec. Indian Mus. 26(pt. 6): 615–625, illus.

1924b. The Bishop Museum Collection of Psyllidae (Homoptera). Proc. Hawaii. Entomol. Soc. 5: 369–370.

1925a. Notes on Hawaiian Psyllidae. *Ibid.*, 6: 27–29.

1925b. Notes on California Psyllidae. *Ibid.*, 6: 30–31.
1925c. The homopterous genus Mesohomotoma (Psyllidae or Chermidae). *Ibid.*, 6: 32–35.
1925d. The genus *Macrohomotoma* (Psyllidae or Chermidae). *Ibid.*, 6: 36–39, illus.
1925e. Notes on Psyllidae. Philippine J. Sci. 28: 39–43, illus.
1925f. Psyllidae of South America. Broteria (ser. zool.) 22 (fasc. 2): 56–74, illus.
1927a. Insecta of Samoa, Psyllidae (Chermidae). pt. 2 (fasc. 1): 29–33, illus.
1927b. Psyllidae of Molokai. Proc. Hawaii. Entomol. Soc. 6: 423–424.
1928a. A new psyllid from Maui. *Ibid.*, 7: 33.
1928b. Psyllidae of Fiji and Samoa. *Ibid.*, 7: 33–35.
1928c. Fauna sumatrensis (Beitrage Nr. 61). Psyllidae. Entomol. Mitt. 17: 425–426, illus.

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Senekerim Mardiros Dohanian

1889–1972

Senekerim Mardiros Dohanian was born in Malatia, Armenia on Oct. 12, 1889. He died in Oakland, California on Feb. 17, 1972. His family called him "Sennie," but to me and most of his friends he was known as "Doh."

After the massacres of 1895 when Doh's father and grandfather were killed, his mother sent him to an orphanage for a year in Brusa. During the next 5 years his remarkable mother disposed of her possessions, organized her small family and left Malatia in the fall of 1902 for America. Mother, grandmother, uncle and three brothers left the seaport of Alexandretta in an English freighter, sailed to Marseilles and headed for America by way of Paris and a Holland American steamship.

The Dohanian family settled in Sommerville, Massachussetts, and Senekerim attended the public schools, graduating from Sommerville High School in 1909. During these years he earned money for college by selling newspapers, shining shoes, washing dishes, as a short order cook on Cape Cod and caddy master at a golf course in the White Mountains.

He entered the Massachussetts College of Agriculture in Amherst in the fall of 1909 but soon transferred to Tufts College, which was closer to home. He received his B.S. degree from Tufts in 1913 and continued at Harvard University School of Forestry in Petersham, Mass. He received the degree of M.S. from Harvard in 1915 and was soon employed by the U. S. Department of Agriculture as an entomologist until he enlisted in the U. S. Air Force in 1917. He was stationed in Texas and was in the Medical Corps in charge of sanitation at Kelly Field until his discharge on 21 Jan. 1919.

Dohanian then returned to the U. S. Department of Agriculture and was sent to Europe in 1924 to collect parasites to fight the gypsy moth. He established a laboratory in Madrid for the care and shipment of these parasites, and while there his laboratory was visited by King Alfonso XIII and Queen Isabella of Spain. I arrived in Madrid in the Spring of 1924 and was delighted to find my old friend already there. We had a number of visits in the next month or so. I was pleased to find he had established excellent relations with Dr. Manuel Aulló, Head of Forest Insect Control for the Spanish Government. Dr. Aulló complimented him to me for his good command of the Spanish language. This had been accomplished by Doh taking a room in a house with several University students with whom he ate breakfast and as many other meals as possible.

Just off Puerto del Sol we found a little semi-basement German restaurant with black and red table cloths which served black bread, cheese, dill pickles and other German food and good cold tap beer. My wife also enjoyed this occasional change from local food.

Following Spain, he travelled to a number of countries including France, Germany, Italy, and Austria, and he was in Portugal during its revolution. His next assignment was in South America to collect parasites to combat the sugar cane

borer (*Diatraea*) for introduction into Puerto Rico. He was in Trinidad, Peru, and British Guiana and he finished this 5-year project in 13 months. During 1925–1926 he was Entomologist for the American Cyanamid Company of New York City.

Then again for the U. S. Department of Agriculture Doh was sent to Eugene, Oregon to establish a laboratory designed to save the filbert trees from attack by insects and to study their parasites. This assignment lasted from 1937 to 1947, following which he was transferred to the International Airport in New York City as inspector of flowers, fruits, and meats from foreign countries. This was an exacting job with long and irregular hours, but he performed it faithfully in his characteristically thorough manner. He retired from government service in 1960 after about 43 years of distinguished and devoted service often performed under difficult circumstances both here and abroad.

Although Dohanian made his home at the residence of his 5-year-younger brother, Luke M. Dohanian, South Newbury, N. H., he spent most of the winters in Arizona and California because of health reasons. He was of course an excellent and intelligent collector of insects. During his several winters in Arizona he made a number of collections of aphids for me. The data for each collection were fully given and clearly written. Although I was able to determine only 11 aphids to species, it appears that little else is known of the aphids of Arizona.

Although in the earlier years of our acquaintance I used to see Doh fairly frequently, I had not seen him for a number of years prior to his death. I recall him as rather slight and energetic with black hair and moustache and with a very friendly though retiring manner. He never married and was thoroughly dedicated to his work. He had a droll sense of humor which I always enjoyed.

Doh was a member of the American Association for the Advancement of Science (Fellow), Entomological Society of America, Entomological Society of Washington (1928), California Academy of Science (1966), Oregon Academy of Science and Oregon Entomological Club, and the Cambridge Entomological Club (V. P. in 1928). He was a member of the fraternal order of John Abbot Lodge AF and AM of Sommerville, Mass.—a 51-year Mason at the time of his death. In April 1942 Dohanian and I attended sessions of the Third International Congress of Malaria and Tropical Medicine in Washington, D. C.

In the Dohanian file in the Tufts Alumni Office is a memo, possibly in his handwriting, which says he was the author of 19 entomological papers and bulletins, several of which are of considerable economic importance. However I can find record of only the following titles attributable to him:

1935. The European corn borer on Long Island. Psyche 41(4): 214–220. (Dec. 1934, but apparently published in Jan. 1935.)

1937. Life history of the thrips parasite *Dasycampus parvipennis* Gahan and the technique for breeding it. J. Econ. Entomol. 30(1): 78–80.

1937. The introduction of parasites of the sugarcane borer into Puerto Rico. J. Agr. Univ. Puerto Rico 21(2): 237–241.

1938. La busqueda de insectos beneficiosos en los tropicos americanos para introducirlos en Puerto Rico. Rev. Agr. Puerto Rico 30(3): 408–412.

1940. *Melissopus lateriferreanus* as a pest of filberts in the Northwest. J. Econ. Entomol. 33(6): 852–856.

1942. Parasites of the filbert worm. J. Econ. Entomol. 36(6): 836–841.

1942. Variability of diapause in *Melissopus latiferreanus*. J. Econ. Entomol. 35(3): 406–411.

1927. Muesebeck, C. F. W., and S. W. Dohanian. A study in hyperaparasitism, with particular reference to the parasites of *Apanteles melanoscelus* (Ratzeburg). U. S. Dept. Agr., Dept. Bull. 1487: 1–35.

Mortimer D. Leonard
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Alfred J. Zmuda

Alfred J. Zmuda, a senior geophysicist of the Johns Hopkins University Applied

Physics Laboratory, died in July at the age of 53 after an apparent heart attack.

Dr. Zmuda, a native of Shenandoah, Pa., received his B.S. degree from St. Francis College in Pennsylvania and his Ph.D. in physics from Catholic University in 1951. During World War II he served in the Marine Corps in the South Pacific.

Dr. Zmuda, a specialist in geomagnetism, ionospheric physics and space physics, made studies with research satellites which led to a better understanding of the earth's magnetic field and the causes of the Northern Lights. He had worked for Johns Hopkins since 1951.

A pioneer in developing theoretical models of the earth's magnetic field, Dr. Zmuda discovered electric currents flowing along the magnetic field lines in the auroral zones of the earth. He also studied high energy particles from solar flares which appear over the polar regions, and he developed the ionospheric models to explain the communications blackouts that often accompany such events.

He published more than 50 scientific papers.

In recent studies Dr. Zmuda explained that the electrical currents that form the outer boundaries of the Van Allen belt flow down along lines of the magnetic field of the earth to collide with the constituents of the atmosphere, resulting in light emissions described as the Aurora Borealis or Northern Lights.

Dr. Zmuda was a former president of the American Geophysical Union's section on geomagnetism and paleomagnetism. He was secretary general of the World Magnetic Survey Board of the International Association of Geomagnetism and Aeronomy and was editor of the program in 1971.

He also was a member of the International Scientific Radio Union, Washington Academy of Sciences and the Philosophical Society. In the early 1960s he was a consultant to the geophysics panel of the Scientific Board of the Air Force.

He leaves his wife, the former Margaret Koval; two daughters Carole Ann of Silver Spring and Mrs. Mary Alice Lutty of the District; his father, Frank L.; a sister, Mrs. Ruth O'Neill of Shenandoah, and three brothers, Robert of Kensington, Frank of Rockville and Richard, of Hershey, Pa.

NOTICE

1974–1975
FALL/WINTER PROGRAM
MAIN AUDITORIUM, ADMINISTRATION BUILDING
AGRICULTURAL RESEARCH CENTER
U.S. DEPARTMENT OF AGRICULTURE
BELTSVILLE, MARYLAND

Wednesday Oct. 9, 1974 2:30 p.m.	Topic: Speaker:	Chemical Signals in the Development of Slime Molds Dr. John T. Bonner Princeton University Princeton, New Jersey
Wednesday Nov. 20, 1974 1:30 p.m.	Speaker:	Dr. Philip Handler President, National Academy of Sciences (topic to be announced)
Wednesday Dec. 4, 1974 2:30 p.m.	Topic: Speaker:	The Vitamin D-based Endocrine System Dr. Hector F. DeLuca University of Wisconsin Madison, Wisconsin
Wednesday Jan. 15, 1975 2:30 p.m.	Topic: Speaker:	Oxygen Radicals, Oxygen Toxicity and the Superoxide Dismutases Dr. Irwin Fridovich Duke University Durham, North Carolina

Sponsored by
The Graduate School, U.S. Department of Agriculture
and
Beltsville Agricultural Research Center

1974–1975 BARC OVERVIEW

A series of seminars designed to acquaint the scientific community with the many varied research programs in progress at the Beltsville Agricultural Research Center:

Wednesday Sept. 18, 1974 2:30 p.m.	Topic: Speaker:	Research Activities in the Nutrition Institute Dr. Walter Mertz, Chairman, NI
Wednesday Oct. 30, 1974 2:30 p.m.	Topic: Speaker:	Research Activities in the Agricultural Environmental Quality Institute Dr. Loran L. Danielson, Chairman, AEQI
Wednesday Nov. 13, 1974 2:30 p.m.	Topic: Speaker:	Research Activities in the Agricultural Marketing Research Institute Dr. Essex E. Finney, Jr., Chairman, AMRI
Wednesday Dec. 11, 1974 2:30 p.m.	Topic: Speaker:	Research Activities in the Plant Physiology Institute Dr. Harry R. Carns, Chairman, PPI

Wednesday Jan. 29, 1975 2:30 p.m.	Topic: Speaker:	Research Activities in the Insect Identification and Beneficial Insect Introduction Institute Dr. Lloyd K. Knutson, Chairman, IIBII
Wednesday Feb. 12, 1975 2:30 p.m.	Topic: Speaker:	Research Activities in the Plant Protection Institute Dr. Burton Y. Endo, Chairman, PPI
Wednesday March 12, 1975 2:30 p.m.	Topic: Speaker:	Research Activities in the Animal Parasitology Institute Dr. Frank D. Enzie, Chairman, API
Wednesday April 2, 1975 2:30 p.m.	Topic: Speaker:	Research Activities in the Animal Physiology and Genetics Institute Dr. James W. Smith, Chairman, APGI
Wednesday May 7, 1975 2:30 p.m.	Topic: Speaker:	Research Activities in the Plant Genetics and Germplasm Institute Dr. John G. Moseman, Chairman, PGGI

Directions to Beltsville Agricultural Research Center: take Beltway Exit 27 north (U.S. Route 1) and bear right shortly before first traffic signal in order to cross over to west side of highway. Administration building is center of group of three buildings facing Route 1.

NOTICE

Make a Contribution to your Academy!

The Academy's financial status has been carefully reviewed and the Board of Managers has voted to increase the annual dues effective on January 1, 1975. The increase to both Fellows and Members is a total of $3.00; two of the dollars are a result of increasing the Journal subscription and the other is added to the dues. This will make the dues as of January 1, 1975, for Fellows $18.00 and Members $13.00.

The increase is necessary to cover the rise in costs for meetings, supplies, services and office management. The previous dues increase, voted in 1970, was adequate for the first two years but not for 1973 and 1974. The major contribution to the 1973 deficit was the markedly reduced income from our mutual funds. The Board is seeking means to improve the investment of the reserve funds.

The increase in dues will generate sufficient income to meet 1975's anticipated expenses. Meanwhile, the deficit will continue for calendar year 1974. The Board of Managers, therefore, is asking all Fellows and Members for a contribution to relieve the pressure on this year's budget. The suggested contribution is $5.00 for Fellows and $2.50 for Members. Your contribution for *any* amount can be made by check payable to the Washington Academy of Sciences, and can be mailed to the Academy office (see inside front cover).

In addition to the search for improving the income from investments the Board has taken measures to reduce expenses, for example, moving the office into a smaller space, thus reducing the rent. Cost consciousness continues to be a major concern.

Tear out this page and mail it with your check today. Your contribution is tax deductible; the Academy's Tax Exempt number is 53-0241911.

Name ____

Address ____

Foreword

The present, 49th issue of the Academy's directory is again this year issued as part of the September number of the Journal. As in previous years, the alphabetical listing is based on a postcard questionnaire sent to the Academy membership. Members were asked to update the data concerning address and membership in affiliated societies by June 14, 1974. In cases in which cards were not received by that date, the address appears as it was used during 1973, and the remaining data were taken from the directory for 1973. Corrections should be called to the attention of the Academy office.

Code for Affiliated Societies, and Society Officers

1 The Philosophical Society of Washington (1898)

President:	George E. Hudson, Code 026, Naval Ordnance Lab., Silver Spring, Md. 20910
Vice-President:	Ralph P. Hudson, NBS, Washington, D.C. 20234
Secretary:	Patricia S. Willis, 2824 W. George Mason Rd., Falls Church, Va. 22042
Delegate:	George E. Hudson

2 Anthropological Society of Washington (1898)

President:	Lawrence Angel, Dept. of Anthropology, Smithsonian Institution, Washington, D.C. 20560
President-elect:	Philleo Nash, Dept. of Anthropology, American Univ., Washington, D.C. 20016
Secretary:	Marjorie G. Whiting, 407 5th St., S.E., Washington, D.C.
Delegate:	Jean K. Boek, Dir., Div. of Special Studies, National Graduate Univ., 3408 Wisconsin Ave., N.W., Washington, D.C. 20016

3 Biological Society of Washington (1898)

President:	Joseph Rosewater, Smithsonian Institution, Washington, D.C. 20560
Secretary:	Richard C. Banks, Smithsonian Institution, Washington, D.C. 20560

4 Chemical Society of Washington (1898)

President:	Alfred Weissler, BF 430, FDA, 200 C St., S.W., Washington, D.C. 20204
President-elect:	Robert F. Cozzens, George Mason Univ., Dept. of Chemistry, Fairfax, Va. 22030
Secretary:	Noel H. Turner, Naval Res. Lab., Code 6171, Washington, D.C. 20375
Delegate:	Robert F. Cozzens

5 Entomological Society of Washington (1898)

President:	Barnard D. Burks, 446 Natural History Bldg., Washington, D.C. 20560
President-elect:	H. Ivan Rainwater, 633A Center Bldg. 1, Hyattsville, Md. 20782
Secretary:	Raymond J. Gagné, W616 Natural History Bldg., Washington, D.C. 20560
Delegate:	None appointed

6 National Geographic Society (1898)

President:	Melvin M. Payne, 17th & M Sts., N.W., Washington, D.C. 20036
Vice-President & Secretary:	Robert E. Doyle, 17th & M Sts., N.W., Washington, D.C. 20036
Delegate:	Alexander Wetmore, Smithsonian Institution, Washington, D.C. 20560

7 Geological Society of Washington (1898)

President:	E. A. Zen, U.S. Geological Survey, Reston, Va. 22092
Vice-President:	Joshua I. Tracey, U.S. Geological Survey, Reston, Va. 22092
Secretary:	David S. Harwood, U.S. Geological Survey, Reston, Va. 22092
Delegate:	Charles Milton, Dept. of Geology, George Washington Univ., Washington, D.C. 20005

8 Medical Society of the District of Columbia (1898)

President:	William S. McCune
President-elect:	Frank S. Bacon
Secretary:	Thomas Sadler
Delegate:	Not appointed

9 Columbia Historical Society (1899)

President:	Hemer T. Rosenberger, 1307 New Hampshire Ave., N.W., Washington, D.C. 20036
Vice-President:	Wilcomb E. Washburn, Smithsonian Institution, Washington, D.C. 20560
Secretary:	Edward F. Gerber, 1233 30th St., N.W., Washington, D.C. 20007
Delegate:	Paul H. Oehser, National Geographic Society, Washington, D.C. 20036

10 Botanical Society of Washington (1902)

President:	Charles R. Gunn, USDA, ARS, Beltsville, Md. 20750
Vice-President:	R.W. Read, Smithsonian Institution, Dept. of Botany, Washington, D.C. 20560
Secretary:	Theodore R. Dudley, U.S. National Arboretum, Washington, D.C. 20225
Delegate:	Conrad B. Link, Dept. of Horticulture, Univ. of Md., College Park, Md. 20742

11 Society of American Foresters, Washington Section (1904)

Chairman:	Carrow T. Prout, Jr., Soil Conservation Serv., USDA, Washington, D.C. 20250
Chairman-elect:	Thomas B. Glazebrook, 7809 Bristow Dr., Annandale, Va. 22003
Secretary:	Murl Storms, 5112 Ampthill Dr., Alexandria, Va. 22313
Delegate:	R. Z. Callaham, 3720 Acosta Rd., Fairfax, Va. 20230

12 Washington Society of Engineers (1907)

President:	Thomas P. Meloy, 6715 Electronic Dr., Springfield, Va. 22151
Vice-President:	George Abraham, 3107 Westover Dr., S.E., Washington, D.C. 20020
Secretary:	Joseph L. Scott, 140 11th St., S.E., Washington, D.C. 20003
Delegate:	George Abraham

13 Institute of Electrical & Electronics Engineers, Washington Section (1912)

Chairman:	Marjorie R. Townsend, 3529 Tilden St., N.W., Washington, D.C. 20008
Vice-chairman:	John J. Kelleher, 3717 King Arthur Rd., Annandale, Va. 22003
Secretary:	Dennis Bodson, 233 N. Columbus St., Arlington, Va. 22203
Delegate:	Harry Fine, 808 Hyde Ct., Silver Spring, Md. 20902

14 American Society of Mechanical Engineers, Washington Section (1923)

Chairman:	Henry M. Curran, Hittman Assoc., Columbia, Md. 21045
Vice-chairman:	Andre H. Gage, PEPCO, 1900 Pennsylvania Ave., N.W., Washington, D.C. 20006
Secretary:	William H. Walston, Jr., Dept. of Mechanical Engineering, Univ. of Md., College Park, Md. 20742
Delegate:	Michael Chi, Dept. of Mechanical Engineering, Catholic Univ., Washington, D.C. 20017

15 Helminthological Society of Washington (1923)

President:	Thomas K. Sawyer, National Marine Fishery Service, Biological Lab., Dept. of Commerce, Oxford, Md. 21654
Vice-President:	Robert S. Isenstein, Animal Parasitology Lab., BARC-East, Beltsville, Md. 20705
Secretary:	William R. Nickle, Nematology Lab., Plant Protection Inst., BARC-West, Beltsville, Md. 20705
Delegate:	James H. Turner, Division of Research Grants, NIH, Bethesda, Md. 20014

16 American Society for Microbiology, Washington Branch (1923)

President:	Lewis F. Affronti, Dept. of Microbiology, The George Washington Univ., 2300 I St., N.W., Washington, D.C. 20037
Vice-President:	Joseph C. Olson, Jr., NIH, Bethesda, Md. 20014
Secretary:	Charles H. Zierdt, NIH, Bethesda, Md. 20014
Delegate:	Lewis F. Affronti

17 Society of American Military Engineers, Washington Post (1927)

President:	Brig. Gen. William Wray, USA, Office Chief of Engineers, Forrestal Bldg., Washington, D.C. 20314
Vice-President:	Cdr. Theodore J. Wojnar, USCG, Coast Guard Hdqtrs., Attn: (ECV), Washington, D.C. 20590
Secretary:	Lt. Col. Ancil R. Pressley, USA, Office Chief of Engineers, Forrestal Bldg., Washington, D.C. 20314
Delegate:	Cdr. Hal P. Demuth, 4025 Pinebrook Rd., Alexandria, Va. 22310

18 American Society of Civil Engineers, National Capital Section (1942)

President:	Floyd D. Peterson, 9627 Hawick Lane, Kensington, Md. 20795
Vice-President:	Philip L. Brach
Secretary:	John B. Roose, 6008 Jennings Lane, Springfield, Va. 22150
Delegate:	Shou Shan Fan, 2313 Glenallen Ave., Silver Spring, Md. 20906

19 Society for Experimental Biology & Medicine, D.C. Section (1952)

President:	Benjamin H. Bruckner, Natl. Inst. for Occupational Safety & Health, Rm. 3-44, Park Bldg., 5600 Fishers Lane, Rockville, Md. 20852
President-elect:	Leon Prosky, 9521 Cherry Oak Ct., Burke, Va. 22015
Secretary:	Juan Penhos, 5402 Surrey St., Chevy Chase, Md. 20015
Delegate:	Donald Flick, 930 S. 19th St., Arlington, Va. 22015

20 American Society for Metals, Washington Chapter (1953)

Chairman:	Klaus M. Zwilsky, U.S. Atomic Energy Comm., Washington, D.C. 20545
Vice-chairman:	Alan H. Rosenstein, Air Force Office of Scientific Res., 1400 Wilson Blvd., Arlington, Va. 22209
Secretary:	Joseph Malz, NASA, Code RWM, Washington, D.C. 20546
Delegate:	Glen W. Wensch, U.S. Atomic Energy Comm., Washington, D.C. 20545

21 International Association for Dental Research, Washington, Section (1953)

President:	F.A. San Filippo, Dental Corps, U.S. Army, Ft. Belvoir, Va. 22060
President-elect:	Robert W. Longton, NMRI, Bethesda, Md. 20014
Secretary:	W.H. Bowen, Extramural Programs, NIDR, Westwood Bldg., Bethesda, Md. 20014
Delegate:	N.H.C. Griffiths, Howard Univ., College of Dentistry, Washington, D.C. 20001

22 American Institute of Aeronautics and Astronautics, National Capital Section (1953)

President:	Philip R. Compton, 6303 Mori St., McLean, Va. 22101
Vice-President:	Jack Suddreth, Code RLC/Aero. Prop. Div., NASA Headquarters, Washington, D.C. 20546
Secretary:	Paul M. Burris, The Boeing Co., 955 L'Enfant Plaza North, S.W., Washington, D.C. 20024
Delegate:	Franklin J. Ross, Deputy for Rqmts., Off. Asst. Sec. of A.F., The Pentagon, Rm. 4E973, Washington, D.C. 20330

23 American Meteorological Society, D.C. Chapter (1954)

Chairman:	Clifford J. Murino, National Science Foundation
Vice-chairman:	James K. Angell, ESSA
Secretary:	Mary Ann Ruzecki, ESSA
Delegate:	None appointed

24 Insecticide Society of Washington (1959)

President:	Richard J. Daum, Federal Bldg., USDA, APHIS, PPQ, Rm. 602, Hyattsville, Md. 20782
President-elect:	Richard C. Back, Union Carbide Corp., Suite 1250, 1730 Pa. Ave., N.W., Washington, D.C. 20006
Secretary:	John W. Neal, USDA, ARS, PGGI, Bldg. 467-C, ARS-East, Beltsville, Md. 20705
Delegate:	Robert J. Argauer, USDA, ARS, AEQI, ARC-East, Bldg. 309, Beltsville, Md. 20705

25 Acoustical Society of America (1959)

Chairman:	John A. Molino, Sound Section, NBS, Washington, D.C. 20234
Vice-chairman:	Charles T. Molloy, 2400 Claremont Dr., Falls Church, Va. 22043
Secretary:	William K. Blake, Naval Ship R&D Ctr., Bethesda, Md. 20034
Delegate:	Gerald J. Franz, 9638 Culver St., Kensington, Md. 20795

26 American Nuclear Society, Washington Section (1960)

Chairman:	Oscar M. Bizzell, Atomic Energy Comm.
Vice-chairman:	Justin L. Bloom, Atomic Energy Comm.
Secretary:	Leslie S. Ayres, Arms Control & Disarmament Agency
Delegate:	None appointed

27 **Institute of Food Technologists, Washington Section (1961)**

Chairman:	Tannous Khalil, Giant Foods, Inc., Landover, Md. 20785
Vice-chairman:	Florian C. Majorack, Food & Drug Adm., Washington, D.C. 20204
Secretary:	Glenn V. Brauner, National Canners Assoc., Washington, D.C. 20036
Delegate:	William Sulzbacher, 8527 Clarkson Dr., Fulton, Md. 20759

28 **American Ceramic Society, Baltimore-Washington Section (1962)**

Chairman:	W. T. Bakker, General Refractories Co., P.O. Box 1673, Baltimore, Md. 21203
Chairman-elect:	L. Biller, Glidden-Dirkee Div., SCM Corp., 3901 Hawkins Point Rd., Baltimore, Md. 21226
Secretary:	Edwin E. Childs, J. E. Baker Co., 232 E. Market St., York, Pa. 17405
Delegate:	None appointed

29 **Electrochemical Society, National Capital Section (1963)**

Chairman:	Murray Rosen, Naval Res. Lab., 4555 Overlook Ave., S.W., Washington, D.C. 20390
First Vice-chairman:	Judith Ambrus, Naval Ordnance Lab., White Oak, Md. 20910
Secretary:	David Flynn, Naval Res. Lab., 4555 Overlook Ave., S.E., Washington, D.C. 20390
Delegate:	David Schlain, P.O. Box 348, College Park, Md. 20740

30 **Washington History of Science Club (1965)**

Chairman:	Richard G. Hewlett, Atomic Energy Comm.
Vice-chairman:	Deborah Warner, Smithsonian Institution
Secretary:	Dean C. Allard
Delegate:	None appointed

31 **American Association of Physics Teachers, Chesapeake Section (1965)**

President:	William Logan, D.C. Teachers College, 2565 Georgia Ave., Washington, D.C. 20001
Vice-President:	Eugenie V. Mielczarek, George Mason Univ., 4400 University Dr., Fairfax, Va. 22030
Secretary:	John B. Newman, Towson State College, Towson, Md. 21204
Delegate:	Bernard B. Watson, 6108 London Lane, Bethesda, Md. 20034

32 **Optical Society of America, National Capital Section (1966)**

President:	Irving H. Malitson, NBS, Physics 266-A, Washington, D.C. 20234
Vice-President:	Barton J. Howell, Code 941, Goddard Space Flight Ctr., Greenbelt, Md. 20771
Secretary:	William B. Fussell, NBS, Physics, 203-A, Washington, D.C. 20234
Delegate:	Irving Malitson

33 **American Society of Plant Physiologists, Washington Section (1966)**

President:	William R. Krul, USDA, Plant Hormone & Reg. Lab., Plant Industry Station, Beltsville, Md. 20705
Vice-President:	Bert Drake, Smithsonian Radiation Biology Lab., 12441 Parklawn Dr., Rockville, Md. 20852
Secretary:	Aref Abdul-baki, USDA, Admin. Bldg., Agr. Res. Ctr., W., Beltsville, Md. 20705
Delegate:	W. Shropshire, Jr., Smithsonian Radiation Biology Lab., 12441 Parklawn Dr., Rockville, Md. 20852

34 **Washington Operations Research Council (1966)**

President:	Donald Gross, The George Washington Univ., Washington, D.C. 20005
President-elect:	Frank Trippi, Naval Facilities Eng. Command, Alexandria, Va. 22313
Secretary:	Craig C. Sherbrooke, 8413 Kingsgate Rd., Potomac, Md. 20854
Delegate:	John G. Honig, 7701 Glenmore Spring Way, Bethesda, Md. 20034

35 **Instrument Society of America, Washington Section (1967)**

President:	Francis C. Quinn
President-elect:	John I. Peterson
Secretary:	Frank L. Carou
Delegate:	None appointed

36 American Institute of Mining, Metallurgical & Petroleum Engineers (1968)

Chairman:	Robert W. Ageton, Securities & Exchange Comm., 500 N. Capitol St., Washington, D.C.
Vice-Chairman:	William L. Lennemann, AEC, Washington, D.C. 20545
Secretary:	Reno Masiello, Southern Railway System, P.O. Box 1808, Washington, D.C. 20013
Delegate:	None appointed

37 National Capital Astronomers (1969)

President:	John A. Eisele, 3310 Curtis Dr., No. 202, Hillcrest Heights, Md. 20023
Vice-President:	Henning E. Leidecker, 4811 Avondale Rd., Washington, D.C. 20018
Secretary:	Estelle Finkle, 939 26th St., N.W., Washington, D.C. 20037
Delegate:	John A. Eisele, 3310 Curtis Dr., No. 202, Hillcrest Heights, Md. 20023

38 Maryland-District of Columbia and Virginia Section of Mathematical Assoc. of America (1971)

Chairman:	Geraldine A. Coon, Goucher College, Baltimore, Md.
Secretary:	John Smith, George Mason College, Fairfax, Va.
Delegate:	None appointed

39 D.C. Institute of Chemists (1973)

President:	Kelso B. Morris, 1448 Leegate Rd., N.W., Washington, D.C. 20012
President-elect:	Leo Schubert, 8521 Beech Tree Rd., Bethesda, Md. 20034
Secretary:	Fred D. Ordway, 2816 Fall Jax Dr., Falls Church, Va. 22042
Delegate:	Miloslav Rechcigl, Jr., 1703 Mark Lane, Rockville, Md. 20852

Alphabetical List of Members

M = Member; F = Fellow; E = Emeritus member. Numbers in parentheses refer to numerical code in foregoing list of affiliated societies.

A

AARONSON, STUART A., 1006 Harrison St., Great Falls, Va. 22066 (F)
ABELSON, PHILIP H., President, Carnegie Institution of Washington, 1530 P St., N.W., Washington, D.C. 20005 (F-1, 4, 7, 16)
ABRAHAM, GEORGE, Ph.D., 3107 Westover Dr., S.E., Washington, D.C. 20020 (F-1, 6, 12, 13, 31, 32)
ACHTER, M. R., Code 6306, U.S. Naval Research Lab., Washington, D.C. 20390 (F-20, 36)
ADAMS, CAROLINE L., 242 North Granada St., Arlington, Va. 22203 (E-10)
ADAMS, ELLIOT Q., 1889 Edgewood Dr., Twinsberg, Ohio 44087 (E)
ADLER, SANFORD C., 14238 Briarwood Terr., Rockville, Md. 20853 (M-1)
ADLER, VICTOR E., 8540 Pineway Crt., Laurel, Md. 20810 (M-5, 24)
ADRIAN, FRANK J., Ph.D., Applied Physics Lab., The Johns Hopkins Univ., 8621 Georgia Ave., Silver Spring, Md. 20910 (F)
AFFRONTI, LEWIS, Ph.D., Dept. of Microbiology, George Washington Univ. Sch. of Med., 2300 Eye St., N.W., Washington, D.C. 20037 (F-16)
AHEARN, ARTHUR J., Ph.D., 9621 East Bexhill Dr., Box 294, Kensington, Md. 20795 (F-1)
AKERS, ROBERT P., Ph.D., 9912 Silverbrook Dr., Rockville, Md. 20850 (F-6)
ALBUS, JAMES S., 6100 Westchester, 1406, College Park Md. 20740 (F)
ALDRICH, JOHN W., Ph.D., 6324 Lakeview Dr., Falls Church, Va. 22041 (F-3)
ALDRIDGE, MARY H., Ph.D., Dept. of Chemistry, American University, Washington, D.C. 20016 (F-4)
ALEXANDER, ALLEN L., Ph.D., 4216 Sleepy Hollow Rd., Annandale, Va. 22003 (F-4)
ALEXANDER, BENJAMIN H., Ph.D., 2522 S. Dakota Ave., N.E., Washington, D.C. 20018 (F-4)
ALGERMISSEN, S. T., 3355 Heidelburg Dr., Boulder, Colo. 80303 (F)
ALLEN, ANTON M., 11718 Lakeway Dr., Manassas, Va. 22110 (F)
ALLEN, D. J. FRANCES, Ph.D., 7507 23rd Ave., Hyattsville, Md. 20783 (F)
ALLEN, WILLIAM G., 8306 Custer Rd., Bethesda, Md. 20034 (F-14)
ALTER, HARVEY, Ph.D., Nat. Center for Resource Recovery, Inc., 1211 Connecticut Ave., N.W., Washington, D.C. 20036 (F)
ALTMAN, PHILIP L., 9206 Ewing Dr., Bethesda, Md. 20034 (M)
AMIRIKIAN, ARSHAM, Sc.D., 6526 Western Ave., Chevy Chase, Md. 20015 (F-17, 18)
ANDERSON, FRENCH, Nat. Heart & Lung Inst., Nat. Inst. Health, Bethesda, Md. 20014 (F)
ANDERSON, MYRON S., Ph.D., 1433 Manchester Lane, N.W., Washington, D.C. 20011 (F-4)
ANDERSON, WENDELL L., Rural Rt. 2, Box 2069G, La Plata, Md. 20646 (F-4)
ANDREWS, JOHN S., Sc.D., Animal Parasitology Inst., ARS, Beltsville Agr. Res. Ctr. East, USDA, Beltsville, Md. 20705 (F-15)
ANDRUS, EDWARD D., 1600 Rhode Island Ave., N.W., Washington, D.C. 20036 (M-7, 25)
APPEL, WILLIAM D., B.S., 12416 Regent Ave., N.E., Albuquerque, N. Mex. 87112 (E-6)
APSTEIN, MAURICE, Ph.D., 4611 Maple Ave., Bethesda, Md. 20014 (F-13)
APOSTOLOU, GEORGIA L., 1001 Rockville Pike, #424, Rockville, Md. 20852 (M)
ARGAUER, ROBERT J., Ph.D., 4208 Everett St., Kensington, Md. 20795 (F)
ARMSTRONG, GEORGE T., Ph.D., 1401 Dale Dr., Silver Spring, Md. 20910 (F-1, 4, 6)
ARNOLD KIETH, Ph.D., 6303 Cedell St., Camp Springs, Md. 20031 (F)
ARONSON, C. J., 3401 Oberon St., Kensington, Md. 20910 (M-1, 32)
ARSEM, COLLINS, 10821 Admirals Way, Potomac, Md. 20854 (M-1, 6, 13)
ASLAKSON, CARL I., 5707 Wilson Lane, Bethesda, Md. 20034 (E-1, 6, 12, 18)
ASTIN, ALLEN V., Ph.D., 5008 Battery Lane, Bethesda, Md. 20014 (F-1, 13, 22, 31, 35)
AXILROD, BENJAMIN M., 9915 Marquette Dr., Bethesda, Md. 20034 (F-1)
AYENSU, EDWARD S., Ph.D., 103 G St., N.W., #B219, Washington, D.C. 20024 (F-3, 10)

B

BAKER, ARTHUR A., Ph.D., 5201 Westwood Dr., N.W., Washington, D.C. 20016 (F-7)
BAKER, DONALD J., 9913 Edgehill La., Silver Spring, Md. 20901 (M)
BAKER, LOUIS C.W., Ph.D., Dept of Chemistry, Georgetown University, N.W., Washington, D.C. 20007 (F-4)
BALLARD, L. DOUGLAS, 722 So. Colonial, Sterling, Va. 22170 (F-1, 13, 32)
BARBROW, LOUISE., Natl. Bureau of Standards, Washington, D.C. 20234 (F-1, 13, 32)
BARGER, GERALD L., Ph.D., 209 W. Bayou Dr., Dickinson, Tex. 77539 (F-23)
BARNHART, CLYDE S., Sr., Rt. 4, Box 207A, Athens, Ohio 45701 (F)
BARRETT, MORRIS K., Mrs., Ph.D., 5528 Johnson Ave., Bethesda, Md. 20034 (F-6)
BASS, ARNOLD M., Ph.D., 11920 Coldstream Dr., Potomac, Md. 20854 (F-1, 32)

BEACH, LOUIS A., Ph.D., 1200 Waynewood Blvd., Alexandria, Va. 22308 (F-1, 6)
BEACHAM, LOWRIE M., Jr., U.S. Food and Drug Admin., Rm. 3171, South Bldg., USDA, Washington, D.C. 20250 (F-4, 27)
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Y

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BYLAWS

Washington Academy of Sciences

Last Revised in February 1972

Article I. OBJECTIVES

Section 1. The purposes of the Washington Academy of Sciences shall be: (a) to stimulate interest in the sciences, both pure and applied, and (b) to promote their advancement and the development of their philosophical aspects by the Academy membership and through cooperative action by the affiliated societies.

Section 2. These objectives may be attained by, but are not limited to:

(a) Publication of a periodical and of occasional scientific monographs and such other publications as may be deemed desirable.
(b) Public lectures of broad scope and interest in the fields of science.
(c) Sponsoring a Washington Junior Academy of Sciences.
(d) Promoting science education and a professional interest in science among people of high school and college age.
(e) Accepting or making grants of funds to aid special research projects.
(f) Symposia, both formal and small informal, on any aspects of science.
(g) Scientific conferences.
(h) Organization of, or assistance in, scientific expeditions.
(i) Cooperation with other Academies and scientific organizations.
(j) Awards of prizes and citations for special merit in science.
(k) Maintaining an office and staff to aid in carrying out the purposes of the Academy.

Article II. MEMBERSHIP

Section 1. The membership shall consist of three general classes: members, fellows and patrons.

Section 2. Members shall be persons who are interested in and will support the objectives of the Academy and who are otherwise acceptable to at least two-thirds of the Committee on Membership. A letter or application form requesting membership and signed by the applicant may suffice for action by the Committee; approval by the Committee constitutes election to membership.

Section 3. Fellows shall be persons who by reason of original research or other outstanding service to the sciences, mathematics, or engineering are deemed worthy of the honor of election to Academy fellowship.

Section 4. Nominations of fellows shall be presented to the Committee on Membership as a form approved by the Committee. The form shall be signed by the sponsor, a fellow who has knowledge of the nominee's field, and shall be endorsed by at least one other fellow. An explanatory letter from the sponsor and a bibliography of the nominee's publications shall accompany the completed nomination form.

Section 5. Election to fellowship shall be by vote of the Board of Managers upon recommendation of the Committee on Membership. Final action on nominations shall be deferred at least one week after presentation to the Board, and two-thirds of the vote cast shall be necessary to elect.

Section 6. Each individual (not already a fellow) who has been nominated as a Delegate by a local affiliated society or who has been chosen to be the recipient of an Academy Award for Scientific Achievement shall be considered nominated for immediate election to fellowship by the Board of Managers without the necessity for compliance with the provisions of Sections 4 and 5.

Section 7. An individual of unquestioned eminence may be recommended by vote of the Committee on Membership Promotion for immediate election to fellowship by the Board of Managers, without the necessity for compliance with the provisions of Sections 4 and 5.

Section 8. Persons who have given to the Academy not less than one thousand (1,000) dollars or its equivalent in property shall be eligible for election by the Board of Managers as patrons (for life) of the Academy.

Section 9. Life members or fellows shall be those individuals who have made a single payment in accordance with Article III, Section 2, in lieu of annual dues.

Section 10. Members or fellows in good standing who are retired and are no longer engaged in regular gainful employment may be placed in emeritus status. Upon request to the treasurer for transfer to this status, they shall be relieved of the further payment of dues, beginning with the following January first; shall receive notices of meetings without charge; and at their request, shall be entitled to receive the Academy periodical at cost.

Section 11. Members or fellows living more than 50 miles from the White House, Washington, D.C., shall be classed as nonresident members or fellows.

Section 12. An election to any dues-paying class of membership shall be void if the candidate does not within three months thereafter pay his dues or satisfactorily explain his failure to do so.

Section 13. Former members or fellows who resigned in good standing may be reinstated upon application to the Secretary and approval by the Board of Managers. No reconsideration of the applicant's qualifications need be made by the Membership Committee in these cases.

Article III. DUES

Section 1. The annual dues of each class of members shall be fixed by the Board of Managers. No dues shall be paid by emeritus members and fellows, life members and fellows, and patrons.

Section 2. Members and fellows in good standing may be relieved of further payment of dues by making a single payment to provide an annuity equal to their annual dues. (See Article II, Section 9.) The amount of the single payment shall be computed on the basis of an interest rate to be determined by the Board of Managers.

Section 3. Members or fellows whose dues are in arrears for one year shall not be entitled to receive Academy publications.

Section 4. Members or fellows whose dues are in arrears for more than two years shall be dropped from the rolls of the Academy, upon notice to the Board of Managers, unless the Board shall otherwise direct. Persons who have been dropped from membership for nonpayment of dues may be reinstated upon approval of the Board and upon payment of back dues for two years together with dues for the year of reinstatement.

Article IV. OFFICERS

Section 1. The officers of the Academy shall be a President, a President-elect, a Secretary, and a Treasurer. All shall be chosen from resident fellows of the Academy.

Section 2. The President shall appoint all committees and such non-elective officers as are needed unless otherwise directed by the Board of Managers or provided in the Bylaws. He (or his substitute—the President-elect, the Secretary, or the Treasurer, in that order), shall preside at all meetings of the Academy and of the Board of Managers.

Section 3. The Secretary shall act as secretary to the Board of Managers and to the Academy at large. He shall conduct all correspondence relating thereto, except as otherwise provided, and shall be the custodian of the corporate seal of the Academy. He shall arrange for the publication in the Academy periodical of the names and professional connections of new members, and also of such proceedings of the Academy, including meetings of the Board of Managers, as may appropriately be of interest to the membership. He shall be responsible for keeping a register of the membership, showing such information as qualifications, elections, acceptances, changes of residence, lapses of membership, resignations and deaths, and for informing the Treasurer of changes affecting the status of members. He shall act as secretary to the Nominating Committee (see Art. VI, Sect. 2).

Section 4. The Treasurer shall be responsible for keeping an accurate account of all receipts and disbursements, shall select a suitable depository for current funds which shall be approved by the Executive Committee, and shall invest the permanent funds of the Academy as directed by that Committee. He shall prepare a budget at the beginning of each year which shall be reviewed by the Executive Committee for presentation to and acceptance by the Board of Managers. He shall notify the Secretary of the date when each new member qualifies by payment of dues. He shall act as business advisor to the Editor and shall keep necessary records pertaining to the subscription list. In view of his position as Treasurer, however, he shall not be required to sign contracts. He shall pay no bill until it has been approved in writing by the chairman of the committee or other persons authorized to incur it. The fiscal year of the Academy shall be the same as the calendar year.

Section 5. The President and the Treasurer, as directed by the Board of Managers, shall jointly assign securities belonging to the Academy and indorse financial and legal papers necessary for the uses of the Academy, except those relating to current expenditures authorized by the Board. In case of disability or absence of the President or Treasurer, the Board of Managers may designate the President-elect or a qualified Delegate as Acting President or an officer of the Academy as Acting Treasurer, who shall perform the duties of these officers during such disability or absence.

Section 6. An Editor shall be in charge of all activities connected with the Academy's publications. He shall be nominated by the Executive Committee and appointed by the President for an indefinite term subject to annual review by the Board of Managers. The Editor shall serve as a member of the Board.

Section 7. An Archivist may be appointed by the President. If appointed, he shall maintain the permanent records of the Academy, including important records which are no longer in current use by the Secretary, Treasurer, or other officer, and such other documents and material as the Board of Managers may direct.

Section 8. All officers and chairmen of standing committees shall submit annual reports at the May meeting of the Board of Managers.

Section 9. The Nominating Committee (Article IV, Section 2) shall prepare a slate listing two or more persons for each of the offices of President-elect, of Secretary and of Treasurer, and four or more persons for the two Managers-at-large whose terms expire each year and at least two persons to fill each vacant unexpired term of manager-at-large. The slate shall be presented for approval to the Board of Managers at its first meeting in October. Not later than November 15, the Secretary shall forward to each Academy Member and Fellow an announcement of the election, the committee's nomination for the offices to be filled, and a list of incumbents. Additional candidates for such offices may be proposed by any Member or Fellow in good standing by letter received by the Secretary not later than Dec. 1. The name of any eligible candidate so proposed by ten Members or Fellows shall be entered on the ballot.

Section 10. Not later than December 15, the Secretary shall prepare and mail ballots to members and fellows. Independent nominations shall be included on the ballot, and the names of the nominees shall be arranged in alphabetical order. When more than two candidates are nominated for the same office the voting shall be by preferential ballot in the manner prescribed by the Board of Managers. The ballot shall contain also a notice to the effect that votes not received by the Secretary before the first Thursday of January, and votes of individuals whose dues are in arrears for one year or more, will not be counted. The Committee of Tellers shall count the votes and report the results at the annual meeting of the Academy.

Section 11. The newly elected officers shall take office at the close of the annual meeting, the President-elect of the previous year automatically becoming President.

Article V. BOARD OF MANAGERS

Section 1. The activities of the Academy shall be guided by the Board of Managers, consisting of the President, the President-elect, the immediate past President, one Delegate from each of the affiliated societies, the Secretary, the Treasurer, six elected Managers-at-Large, and the Editor. The elected officers of the Academy shall hold like offices on the Board of Managers.

Section 2. One Delegate shall be selected by each affiliated society. He shall serve until replaced by his society. Each Delegate is expected to participate in the meetings of the Board of Managers and vote on behalf of his society.

Section 3. The Board of Managers shall transact all business of the Academy not otherwise provided for. A quorum of the Board shall be nine of its members.

Section 4. The Board of Managers may provide for such standing and special committees as it deems necessary.

Section 5. The Board shall have power to fill vacancies in its own membership until the next annual election. This does not apply to the offices of President and Treasurer (see Art. IV, Sect. 5), nor to Delegates (see Art. V, Sect. 2).

Article VI. COMMITTEES

Section 1. An Executive Committee shall have general supervision of Academy finances, approve the selection of a depository for the current funds, and direct the investment of the permanent

funds. At the beginning of the year it shall present to the Board of Managers an itemized statement of receipts and expenditures of the preceding year and a budget based on the estimated receipts and disbursements of the coming year, with such recommendations as may seem desirable. It shall be charged with the duty of considering all activities of the Academy which may tend to maintain and promote relations with the affiliated societies, and with any other business which may be assigned to it by the Board. The Executive Committee shall consist of the President, the President-elect, the Secretary and the Treasurer (or Acting Treasurer) ex officio, as well as two members appointed annually by the President from the membership of the Board.

Section 2. The President, with the approval of the Board of Managers, shall appoint a Nominating Committee of six Fellows of the Academy, at least one of whom shall be a past President of the Academy, and at least three of whom shall have served as Delegates for at least one year. The Chairman shall be a past President. (See Article IV, Section 9.)

Section 3. The President shall appoint in advance of the annual meeting an Auditing Committee consisting of three persons, none of whom is an officer, to audit the accounts of the Treasurer (Art. VII, Sect. 1).

Section 4. On or before the last Thursday of each year the President shall appoint a committee of three Tellers whose duty it shall be to canvass the ballots (Art. IV, Sect. 10, Art. VII, Sect. 1).

Section 5. The President shall appoint from the Academy membership such committees as are authorized by the Board of Managers and such special committees as necessary to carry out his functions. Committee appointments shall be staggered as to term whenever it is determined by the Board to be in the interest of continuity of committee affairs.

Article VII. MEETINGS

Section 1. The annual meeting shall be held each year in May. It shall be held on the third Thursday of the month unless otherwise directed by the Board of Managers. At this meeting the reports of the Secretary, Treasurer, Auditing Committee (see Article VI, Sect. 3), and Committee of Tellers shall be presented.

Section 2. Other meetings may be held at such time and place as the Board of Managers may determine.

Section 3. The rules contained in "Robert's Rules of Order Revised" shall govern the Academy in all cases to which they are applicable, and in which they are not inconsistent with the bylaws or special rules of order of the Academy.

Article VIII. COOPERATION

Section 1. The term "affiliated societies" in their order of seniority (see Art. VI, Sect. 2) shall be held to cover the:

Philosophical Society of Washington
Anthropological Society of Washington
Biological Society of Washington
Chemical Society of Washington
Entomological Society of Washington
National Geographic Society
Geological Society of Washington
Medical Society of the District of Columbia
Columbia Historical Society
Botanical Society of Washington
Washington Section of Society of American Foresters
Washington Society of Engineers
Washington Section of Institute of Electrical and Electronics Engineers
Washington Section of American Society of Mechanical Engineers
Helminthological Society of Washington
Washington Branch of American Society for Microbiology
Washington Post of Society of American Military Engineers
National Capital Section of American Society of Civil Engineers
District of Columbia Section of Society for Experimental Biology and Medicine
Washington Chapter of American Society for Metals
Washington Section of the International Association for Dental Research
Washington Section of American Institute of Aeronautics and Astronautics

D.C. Branch of American Meteorological Society
Insecticide Society of Washington
Washington Chapter of the Acoustical Society of America
Washington Section of the American Nuclear Society
Washington Section of Institute of Food Technologists
Baltimore-Washington Section of the American Ceramic Society
Washington-Baltimore Section of the Electrochemical Society
Washington History of Science Club
Chesapeake Section of American Association of Physics Teachers
National Capital Section of Optical Society of America
Washington Section of American Society of Plant Physiologists
Washington Operations Research Council
Washington Section of Instrument Society of America
American Institute of Mining, Metallurgical, and Petroleum Engineers
National Capital Astronomers
Maryland-District of Columbia-Virginia Section of the Mathematical Association of America
District of Columbia Institute of Chemists

and such others as may be hereafter recommended by the Board and elected by two-thirds of the members of the Academy voting, the vote being taken by correspondence. A society may be released from affiliation on recommendation of the Board of Managers, and the concurrence of two-thirds of the members of the Academy voting.

Section 2. The Academy may assist the affiliated scientific societies of Washington in any matter of common interest, as in joint meetings, or in the publication of a joint directory: Provided, it shall not have power to incur for or in the name of one or more of these societies any expense or liability not previously authorized by said society or societies, nor shall it without action of the Board of Managers be responsible for any expenses incurred by one or more of the affiliated societies.

Section 3. No affiliated society shall be committed by the Academy to any action in conflict with the charter, constitution, or bylaws of said society, or of its parent society.

Section 4. The Academy may establish and assist a Washington Junior Academy of Sciences for the encouragement of interest in science among students in the Washington area of high school and college age.

Article IX. AWARDS AND GRANTS-IN-AID

Section 1. The Academy may award medals and prizes, or otherwise express its recognition and commendation of scientific work of high merit and distinction in the Washington area. Such recognition shall be given only on approval by the Board of Managers of a recommendation by a committee on awards for scientific achievement.

Section 2. The Academy may receive or make grants to aid scientific research in the Washington area. Grants shall be received or made only on approval by the Board of Managers of a recommendation by a committee on grants-in-aid for scientific research.

Article X. AMENDMENTS

Section 1. Amendments to these bylaws shall be proposed by the Board of Managers and submitted to the members of the Academy in the form of a mail ballot accompanied by a statement of the reasons for the proposed amendment. A two-thirds majority of those members voting is required for adoption. At least two weeks shall be allowed for the ballots to be returned.

Section 2. Any affiliated society or any group of ten or more members may propose an amendment to the Board of Managers in writing. The action of the Board in accepting or rejecting this proposal to amend the bylaws shall be by a vote on roll call, and the complete roll call shall be entered in the minutes of the meeting.

ACT OF INCORPORATION OF THE WASHINGTON ACADEMY OF SCIENCES

We, the undersigned, persons of full age and citizens of the United States, and a majority being citizens of the District of Columbia, pursuant to and in conformity with sections 545 to 552, inclusive, of the Revised Statutes of the United States relating to the District of Columbia, as amended by an Act of Congress entitled "An Act to amend the Revised Statutes of the United States relating to

the District of Columbia and for other purposes," approved April 23, 1884, hereby associate ourselves together as a society or body corporate and certify in writing:

1. That the name of the society is the Washington Academy of Sciences.
2. That the term for which the Corporation is organized shall be perpetual.
3. That the Corporation is organized and shall be operated exclusively for charitable, educational and scientific purposes and in furtherance of these purposes and for no other purpose shall have, but not be limited to, the following specific powers and purposes:
    a. To encourage in the broadest and most liberal manner the advancement and promotion of science.
    b. To acquire, hold, and convey real estate and other property and to establish general and special funds.
    c. To hold meetings.
    d. To publish and distribute documents.
    e. To conduct lectures.
    f. To conduct, endow, or assist investigation in any department of science.
    g. To acquire and maintain a library.
    h. And, in general, to transact any business pertinent to an academy of sciences.

    Provided, however, that notwithstanding the foregoing enumerated powers, the Corporation shall not engage in activities, other than as an insubstantial part thereof, which are not in themselves in furtherance of its charitable, educational and scientific purposes.
4. That the affairs, funds, and property of the Corporation shall be in general charge of a Board of Managers, the number of whose members for the first year shall be nineteen, all of whom shall be chosen from among the members of the Academy.
5. That in the event of dissolution or termination of the Corporation, title to and possession of all the property of the Corporation shall pass to such organization, or organizations, as may be designated by the Board of Managers; provided, however, that in no event shall any property of the Corporation be transmitted to or vested in any organization other than an organization which is then in existence and then qualified for exemption as a charitable, educational or scientific organization under the Internal Revenue Code of 1954, as amended.

Editor's Note: This Act of Incorporation is shown as amended in 1964 by Francois N. Frenkiel, President, and George W. Irving, Jr., Secretary, acting for the Washington Academy of Sciences, in a Certificate of Amendment notarized on September 16, 1964. A copy of the original Act of Incorporation dated February 18, 1898, appears in the Journal for November 1963, page 212.

JOURNAL OF THE WASHINGTON ACADEMY OF SCIENCES

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CONTENTS (Continued from Front Cover)

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CONTENTS

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Founded in 1898

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All delegates of affiliated Societies (see facing page)

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The Journal

This journal, the official organ of the Washington Academy of Sciences, publishes historical articles, critical reviews, and scholarly scientific articles; proceedings of meetings of the Academy and its Board of Managers; and other items of interest to Academy members. The *Journal* appears four times a year (March, June, September, and December) — the September issue contains a directory of the Academy membership.

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Speculations Concerning the Dependence of Emission Line Contour on Frequency Shift in the Scattering of Monochromatic Radiation

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ABSTRACT

The importance of small red wavelength shifts occurring in the scattering of visible light is discussed with regard to the interpretation of the contour of stellar emission lines.

The observed distribution of the intensity of stellar radiation as a function of wavelength has led to conclusions concerning the structure of the universe. In view of the importance of these conclusions, one should make certain that no possible causes contributing to the form of the contour have been ignored. Small red shifts accompanying scattering, besides those originating in the Doppler effect, have been observed and are discussed theoretically below. Thus if the conditions are such that scattering accompanied by wavelength shifts plays a role in determining the shape of this curve, then the results might be significantly altered from those obtained without considering the contribution to the curve of this particular type of scattered radiation.

Cabanne and Daure (C.R. 186, 1533, 1928) reported a red shift of the Rayleigh lines of the order of 0.06 A in a variety of liquids. This was later confirmed by Vacher (C.R. 191, 1121, 1930). I observed similar data (Phys. Rev. 34, 1061, 1929), namely a red shift in the intense scattering at small angles of light passing through a Kerr cell operated at 300 MHz with water as the dielectric. This effect may arise from the orientation of the water dipole in the field to form a coarse grating as suggested by Dodd and Sanchez (Amer. Phys. Soc. Bull., Ser. II, 13, GF5, 101, 1968). Finally, Singh (Proc. Phys. Soc. London 66A, 309, 1953) showed that Raman lines of CCl_4 excited by the Hg 4358 A line suffered a red shift when an electric field was impressed across the liquid. He suggested that the electric field may have induced an ordered orientation of the liquid molecules modifying the conditions under which scattering occurs. All these observations were made with liquids because the scattering in gases is of very low intensity and the measurements of the line profile tend to be obscured by stray diffused light.

These observations have been largely ignored in calculating the distribution of scattered radiation. Conditions in the stellar atmospheres are often such that multiple scattering predominates. This situation makes it all the more important

[1]Submitted by Mrs. Arthur Bramley as a posthumous contribution from the author.

that the analysis of the scattering process be adequate and all inclusive.

Another relevant aspect which I found (Electron. Letters 3, 266, 1967) is the scattering of ultrashort monochromatic coherent pulses, about 100 wavelengths long, by free electrons. This process occurs as a result of the rectifying interaction of ultrashort pulses with a nonlinear intensity gradient with free electrons in a highly ionized medium. Consider an asymmetric light pulse propagated in the z-direction and polarized in the y-direction. Let the envelope of the absolute value of the electric vector E be triangular in shape as expressed in Eq. (2) of my earlier paper (Electron. Letters 3, 266, 1967). Carrying out the indicated integration over the duration T of the light pulse, we obtain for the change in the momentum M_z of the light pulse

$$M_z = -\frac{e\mu\bar{v}_x}{6\nu}\frac{H_{max}}{N^2}$$

The bar denotes the average value of the electron velocity in the x-direction during the passage of the light pulse. This change in momentum requires the energy of the light pulse to change by an amount $\Delta W = cM_z$. The parameters are shown in Table 1.

It is obvious that in the absence of a third process, the conservation of energy and momentum cannot be maintained in the interaction of a free electron and an electromagnetic pulse.

The interaction of the electron with the ionized medium may be of sufficient strength to make possible a transfer of momentum to a third particle, an ionized atom or molecule, so that the electromagnetic energy change can be assumed by the electron. The other alternative is the radiation of electromagnetic energy by the accelerated electron or, what is probably equivalent, a redistribution of the waveform for the coherent ultrashort pulse to satisfy the conservation of energy consistent with the value for the electromagnetic momentum.

For values of $E_{max} \leq 1.5\ 10^{10}$ v/m, readily obtainable in continuous coherent light beams, a wavemechanical representation is required. However, the rectifying interaction considered here is generated for the typical case $N = 100$, $\lambda = 1\cdot10^{-6}$ m by a pulse 0.1 mm in length. In this region, the classical electromechanical approach should be valid.

The final consideration concerns the magnitude of the energy transferred between the ultrashort light pulse and the electron. This transfer will occur between a single photon and the electron.

On the basis of the parameters listed in Table 1, we obtain $\Delta\nu/\nu = 1\cdot10^{-5}$. The agreement between this result and the experimental data cited above is purely coincidental. In stellar radiation processes, possible values of v as high as 10^8 m/sec have been considered. The value of $E_{max} = 1.6\cdot10^5$ v/m is in the lower range for E_{max} in ultrashort pulses produced in the laboratory. It it is assumed that $\bar{v} \sim 5\cdot10^7$ m/sec and $E_{max} \sim 3\cdot10^6$, then $\Delta\nu/\nu$ is $1\cdot10^{-2}$.

If collisions are to play a significant role in the rate of adjustment of momentum and energy, then the lower limit for the density of electrons and atoms or molecules in an ionized medium is 10^{13} per m^3. Since in this process the radiation from the accelerated electron is emitted at the collision with an ionized atom or molecule, the radiation pattern resulting from the interaction of an ultrashort coherent pulse with a free electron will approach that of an antenna, provided the width of the coherent light beam is much larger than $N\lambda$.

Table 1. Terminology and Typical Values for the Parameters

Parameter	Symbol	Typical value
Velocity of light	c	$3\cdot10^8$ m/sec
Magnetic field, maximum absolute value	H_{max}	$0.4\cdot10^3$ amp-turn/m
Average electron velocity in x-direction	$\bar{v}_x$	$1\cdot10^6$ m/sec
Frequency of light	ν	$3\cdot10^{14}$ sec^{-1}
Number of wavelengths per pulse	N	100

The wavelength shift on scattering may well play a significant role in the interpretation of stellar observations. In the measurements by Wilson, discussed by Unsöld ("Der neue Kosmos," 195, Springer, Heidelberg, 1967), the half-width of the emission lines of stars of spectral type G to M* was found to be substantially greater than expected. The observations on the contour of the emission lines fit in with the data presented in this note. In the case of multiple scattering of a high order, the contour of an emission line is fundamentally changed. This does not represent an absorption of energy but a displacement of energy in the blue towards the red end of the spectrum.

Zn and Cu Levels in the Eastern Oyster, Crassostrea virginica, *From the Lower James River*

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ABSTRACT

Levels of Zn and Cu in shucked, whole bodies of the oyster *Crassostrea virginica* ranged up to 10,000 ppm Zn and 584 ppm Cu, with levels averaging 3915 ppm Zn and 180 ppm Cu. These levels are near the upper limits of values previously reported for the species and are considerably higher than levels reported in a previous survey of the same area. The data may indicate increased Zn contamination of lower James River oysters over the period 1971–1973.

Live oysters were collected on 27 June 1973 from a rock retaining wall along Craney Island, a large man-made peninsula extending into the lower James River (Fig. 1). After shucking, the oyster body was dried to a constant weight at 105° C. and digested using nitric and perchloric acids (Baumhardt and Welch, 1972; Anderson, 1972). Zn and Cu concentrations were determined using atomic absorption spectrophotometry and results are expressed on the basis of μgm metal/gm dry oyster tissue. A total of 104 oysters was sampled. These were pooled in order to provide sufficient material for analysis, yielding 50 pairs of Zn and Cu concentrations (Fig. 2).

As a check on analytical procedure, replicate samples of standard bovine liver were carried through the entire digestion analysis procedure. Results of these determinations, shown in Table 1, indicate generally good agreement with the prescribed National Bureau of Standards values. The fact that observed levels were slightly below the prescribed concentrations may mean that levels recorded for the oysters may be slight underestimates.

Discussion

The bioaccumulation of heavy metals, especially Zn and Cu, by *Crassostrea virginica* has been well documented (Hiltner and Wichmann, 1919; Hunter and Harrison, 1928; McFarren *et al.*, 1962; Galtsoff, 1964; Pringle *et al.*, 1968; Shuster and Pringle, 1969; Kopfler and Moyer, 1969; Pequegnot *et al.*, 1969; Wolfe, 1970; Windom and Smith, 1972; and Bender *et al.*, 1972).

In their survey of Virginia estuaries, including the area sampled in this study, Bender *et al.* (1972) advanced a linear relationship between Zn and Cu content in oysters from areas without unnatural inputs of either of the metals. A 95%

Fig. 1. Craney Island study site, lower James River.

confidence band about this least squares regression line (Y = 1.9 + 0.09X) was described by Y = −33 + 0.07X and Y = + 30 + 0.11X. Points lying either below or above this confidence band indicate unnatural contamination of oysters by Zn or Cu, respectively. Zn and Cu levels in oysters from this study are described by the equation Y = −31 + 0.05X, indicating increased Zn contamination (Fig. 3).

Several factors may be advanced to explain the observed increase in Zn levels in lower James River oysters over the period 1971 to 1973. First, the chemical methods utilized to digest the oyster tissue differed slightly. Bender *et al.* (1972) employed a nitric acid process, while I used a nitric and perchloric acid digestion. Secondly, part of the increase in Zn levels might be ascribed to seasonal changes in the elemental composition of the oyster (Galtsoff, 1953). Bender *et al.* collected oyster samples from February to March, while I sampled in June. Thirdly, the study by Bender *et al.*, being a survey report of

Table 1. Analysis of standard bovine liver.

	Zn (ppm)	Cu (ppm)
National Bureau of Standards	130 ± 10	193 ± 10
This study	108 ± 6.0	180 ± 5.0

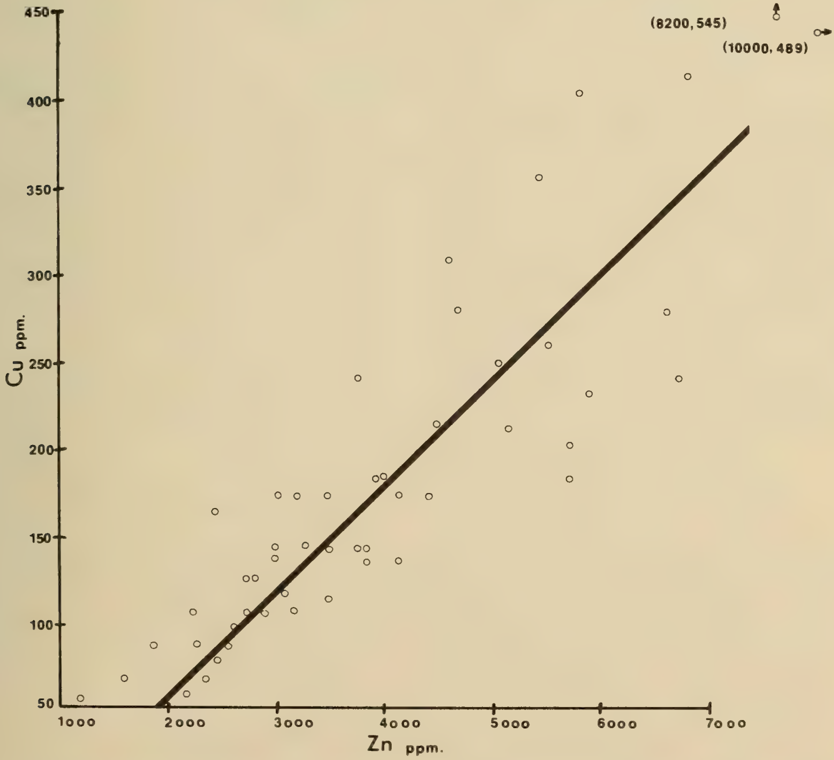

Fig. 2. Relationship between zinc and copper in oysters from the lower James River.

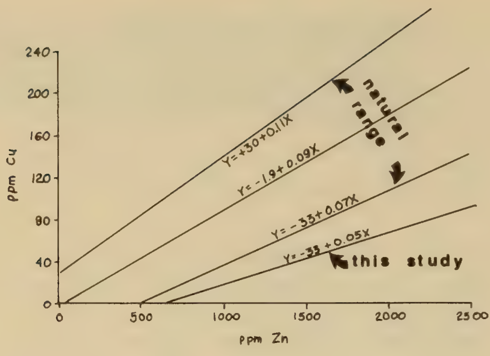

Fig. 3. Oyster data indicating Zn contamination, after Bender, Huggett, and Slone (1972). The natural range represents the 95% confidence interval for Zn and Cu levels in oysters from uncontaminated Virginia estuaries.

general conditions in several Virginia estuaries, may not have had sampling points dense enough to detect this local area of higher contamination. Lastly, the difference between the Zn levels reported for 1971 and those observed in 1973 may reflect a real increase in Zn pollution of this waterway.

Summary

A survey of Zn and Cu in oysters inhabiting the lower James River was conducted and results of the analysis compared to a previous survey of the same area. A substantial increase in Zn content of the oysters was observed over the 2-1/2 year period between the studies. An increase in the Zn contamination of the waters and/or seasonal variation in the elemental composition of the oyster are probable causes.

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Gaudiella minuta, *A New Genus and Species of Mite (Acarina: Acaridia) Belonging to the New Family Gaudiellidae*[1,2]

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ABSTRACT

A new family, genus, and species, Gaudiellidae, *Gaudiella minuta*, is described from a stingless bee, *Melipona quadrifasciata* Lep., from Brazil.

Originally, we had intended to publish a new genus and species, but to assign the unique specimen being studied, it was necessary to delve into the higher classification of the Astigmata. In this study we discovered an interesting method of partially defining some of the higher categories with both morphological and biological characterizations.

Utilizing the suprafamial groups of Krantz (1970) as an example, the suborder Astigmata (Acaridiae of authors) is divided into 2 supercohorts—the Acaridia with the superfamilies Anoetoidea, Canestrinoidea, and Acaroidea, and the supercohorts Psoroptidia with the remaining superfamilies including Ewingoidea, Psoroptoidea, Analgoidea, and Sarcoptoidea. Using the chaetotaxal and solenidiotaxal signatures of Grandjean (1939), the following characters can be used to separate certain suprafamilial taxa without resorting to host data or pretarsal modifications:

1. Supercohort Acaridia, superfamily Anoetoidea: tibia I with two ventral setae (*gT*, *hT*) and solenidion φ; venter with 2 pairs of large ring structures not associated with the genital region; genital discs large.
2. Supercohort Acaridia, superfamily Acaroidea: tibia I with 2 ventral setae and solenidion φ; venter with 2 pairs of well-developed genital discs associated with genital region.
3. Supercohort Acaridia, superfamily Canestrinoidea: tibia I with only solenidion φ, without ventral setae; venter with 2 pairs of well-developed discs associated with region.
4. Supercohort Psoroptidia, superfamily Ewingoidea: tibia I with 2 ventral setae and with atrophied genital discs; all other superfamilies (we have not examined all families): tibia I with 1 ventral seta (*gT*) and solenidion φ; venter without genital discs or with atrophied genital discs associated with genital region.

The new taxon, *Gaudiella minuta*, is adequately distinct to be considered to represent a new family in the Acaroidea, Gaudiellidae. Characters are the maximal leg chaetotaxy of Grandjean (1939), the external morphology of Knülle (1959), and the idiosomal chaetotaxy of Atyeo and Gaud (1966). The mite has features typical of the Acaridia and others common to most Psoroptidia but with the following differences:

1. Well-developed genital discs associated with the genital region.
2. Two pairs of setae ventrolateral on tibia I.
3. Four pairs of lyrifissures (lyriform pores), of which 3 pairs are on the dorsal idiosoma and one pair is subterminal, lateral to the anal slit.
4. Many pairs of setae surrounding the anal slit. In the Psoroptidia, there are usually 1 or 2 pairs.
5. Seta *wF* on tibia IV. Although this seta occurs in the Psoroptidia, it is very rare.
6. Setae *u, v* and *p,q* at the apices of the tarsi. In the Psoroptidia (and many Acaridia), these setae are absent or only *p* and *q* are present.

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[2]Published by permission of the Director, New York State Science Service, Journal Series No. 168.

7. Found on insects—common hosts for parasitic or phoretic forms of the Acaridia.

These differences from the generalized Acaroidea usually would not be sufficient for the establishment of a new family. Singly, some of these differences can be found in known taxa, but together the unique morphological modifications give sufficient evidence for establishing the family Gaudiellidae. The following characteristics are distinctive of this new family.

1. The disc-shaped ambulacrum supported by a short stalk has only one counterpart in the supercohort Acaridia, namely, the Hypoderidae (Hypodectidae) (see Fain and Bafort, 1967). The Hypoderidae, subcutaneous parasites of birds in the deutonymphal stage, have a very reduced ambulacral disc reminiscent of the Sarcoptidae or similar to a clawless Glycyphagidae. The ambulacrum of the Gaudiellidae is similar to many of the taxa of the supercohort Psoroptidia, especially some of the Psoroptoidea and Analgoidea.

2. The structure of the oviporus is unique (fig. 2), although most components can be homologized with those of other Acaroidea (compare with *Glycyphagus destructor* (Schrank) as illustrated by Knülle, 1959, fig. 33).

3. The relative positions of the three dorsal lyrifissures and dorsal idiosomal setae are unique. In species that we are familiar with the general pattern of lyrifissures and setae can be related to those of *Acarus siro* L. as illustrated by Knülle (1959, fig. 20). In *Gaudiella minuta*, regardless of the interpretation of the setal pattern, there is little resemblance between the two conditions. One pair of dorsal hysterosomal setae is absent (either *1*, *2* or *h*), and 1 pair of lyrifissures is almost middorsal in position in *G. minuta*.

4. There are no dorsal hysterosomal glands (opisthonotal glands), or is there evidence of a vestigeal pore.

5. The sejugal suture is absent while there is a deep furrow on the prodorsal shield—almost a tectum.

6. An invagination lateral to the anterior genital setae is situated at the mesal termination of a thin, horizontal apodeme. This invagination could be the opening to a ventral hysterosomal gland; such a gland has been observed in males of 2 undescribed species of feather mites (Analgidae: Xolalginae).

7. Certain setae and solenidia are lacking from leg I, namely, *aa*, $\omega 2$, $\sigma 2$, and the famulus. These deficiencies are not unique, only indicative of a trend for reduction found in other Astigmata.

Family **Gaudiellidae**, new family

Diagnosis.—Small acaroid mites at present associated with stingless bees (*Melipona quadrifasciata* Lep.). Female with small disc-shaped ambulacra supported on short stalks. Dorsal hysterosoma lacking external vertical setae as well as 1 pair of dorsal setae and sejugal suture; 3 pairs of lyrifissures present, 1 of which is middorsal in position. Ventral idiosoma with reduced, simple coxosternal skeleton; midventral oviporous Y-shaped and partially covered by integumental flaps; numerous setae and 1 pair of lyrifissures near anal slit. Legs 5-segmented, each ending in a small disc-shaped ambulacrum on a short stalk; tarsi with setae *u, v, p, q*; tibia I with setae *hT*, *gT*; femur IV with *wF*.

Type-genus.—Gaudiella, new genus.

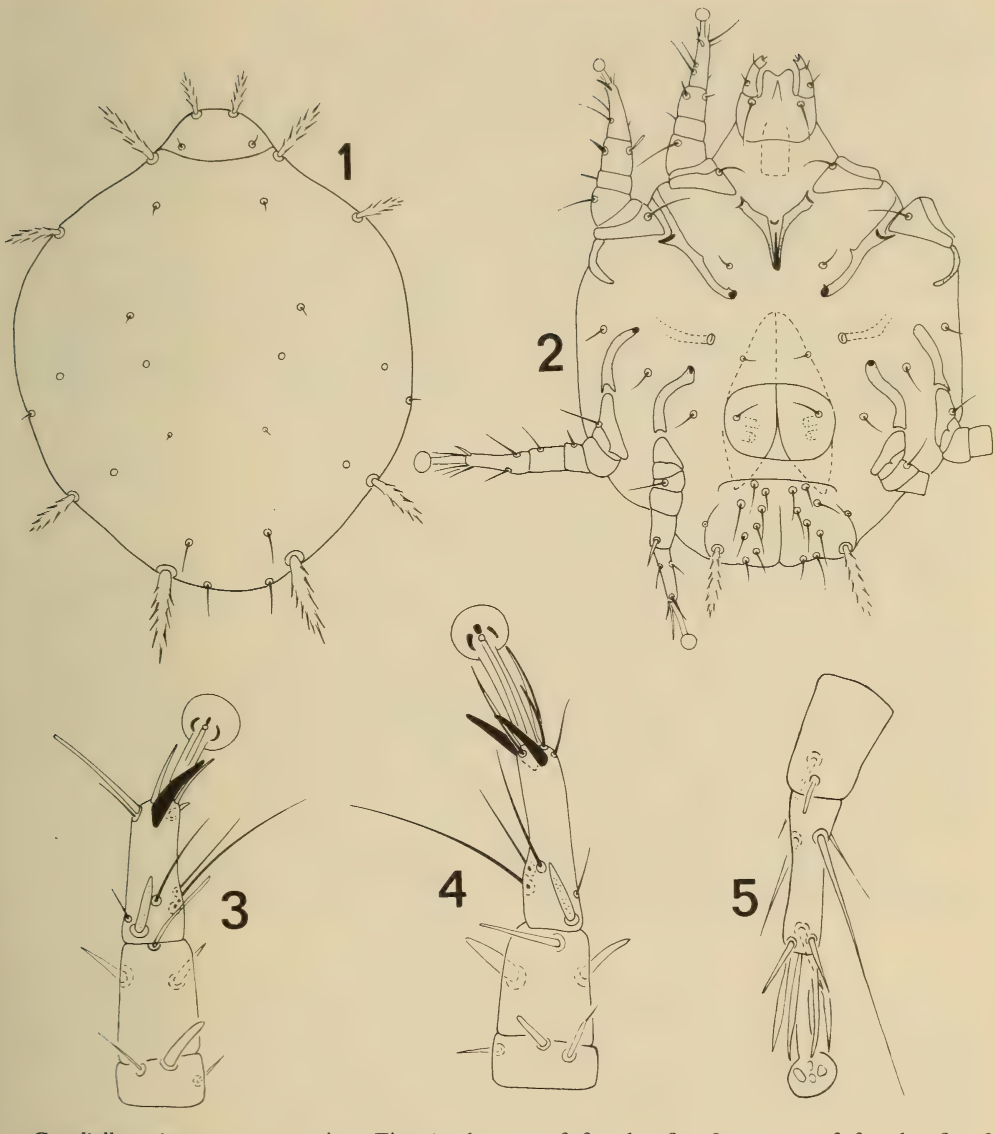
Genus **Gaudiella**, new genus

Diagnosis.—Acaroid mite parasitic (or phoretic) on South American stingless bees. Female with epimerites I fused, other epimerites simple; coxal fields I–IV open. Oviporus Y-shaped, partially covered by flaps; 2 pairs of genital setae, 2 pairs of well-developed genital discs. Anus subterminal, flanked by numerous pairs of anal and adanal setae subequal in length. Dorsum with antero- and poster-lateral setae enlarged, coarsely branched, with deep suture on prodorsum. Legs 5-segmented, each bearing small stalked ambulacral disc rather than empodial claw.

Type-species.—Gaudiella minuta, new species.

Gaudiella minuta, new species
(Figs. 1–5)

Female (holotype).—Small, ovoid with idiosoma 204 μ in length, 163 μ in width, covered by lightly sclerotized shields without striae. Dorsal idiosoma with prominent suture between rows of scapular setae; 3 pairs of lyrifissures (lyriform pores); setae *vi, sci, l 1, l 3, l 4, 1 5* enlarged



Gaudiella minuta, new species. Fig. 1, dorsum of female; fig. 2, venter of female; fig. 3, tarsus-tibia-genu of leg I; fig. 4, tarsus-tibia-genu of leg II; fig. 5, tarsus-tibia of leg IV.

with coarse branchings, other setae simple; setae ve, *l 2* (or *h*) lacking. Ventral idiosoma with Y-shaped epimerites I, other epimerites simple, slightly curved; all coxal fields open; remnant of epimerite III mesally with possible gland opening. Oviporus Y-shaped, covered anteriorly and posteriorly by flaps; posterior genital setae and coxal IV setae form transverse line. Anal slit flanked by 8 pairs of setae (anals, adanals) plus setae *d 5*, *l 5* and ventral lyrifissures.

Legs with pretarsi stalked with simple ambulacra; each ambulacral disc with 2 small unguiform sclerites flanking divided central sclerite. Chaetotaxy of legs I–IV as follows: trochanters, 1-1-1-0; femora, 1-1-0-1; genua, 2-2-1-0; tibiae, 2-2-1-1, tarsi, 9-11-10-10. Solenidiotaxy: genua, 1-1-1-0; tibiae, 1-1-1-1; tarsi, 2-1-0-0. Tarsus I with setae *u* fused with *p* and *v* fused with *q*; setae lacking from maximal complement: *f, aa,* famulus. Tarsi II–IV with setae *p, q, u, v* independent.

Male.—Unknown.

Type Data.—Holotype, female deposited in the Department of Zoology, Universidade de São Paulo, Piracicaba, Brasil, ex *Melipona quadrifasciata* Lep., Ribeirão Preto, São Paulo, Brasil, De-

partment of Genetics, Faculdade de Medicina, October 1973, Dr. Velthuis (coll.), sent by H. Shimanuki of the Bee Laboratory, USDA, Beltsville, Maryland.

Remarks.—This mite is named for Dr. Jean Gaud, Laboratoire de Parasitologie, Faculté de Médicine, 35000-Rennes, France.

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A New Species of Coccipolipus *Parasitic on the Mexican Bean Beetle (Acarina: Podapolipidae)*

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ABSTRACT

Coccipolipus epilachnae n. sp. is described and illustrated. Observations on the biology of the mite are discussed. The mite causes reduction in egg production of the Mexican bean beetle, *Epilachna varivestris* Mulsant.

I am describing a new species of *Coccipolipus* that was associated with the Mexican bean beetle, *Epilachna varivestris* Mulsant (Coccinellidae). Husband (1972) erected the genus *Coccipolipus* for *C. macfarlanei* Husband, which was found associated with the coccinellid *Cycloneda sanguinea* (L.). Feldman-Muhsam and Havivi (1972) described *Podapolipus (Bakerpolipus) coccinellae*, which was collected from the underside of the elytra of *C. sanguinea* together with the fungus *Hesperomyces*. They did not report adverse affects caused by the mite or fungus.

Coccipolipus epilachnae, new species
(Figs. 1–5)

According to Husband's (1972) key to species of *Coccipolipus* (which contains 4 species), *C. epilachnae* is more closely related to *C. macfarlanei* Husband than the other species of the genus. *C. epilachnae* can be separated from *C. macfarlanei* by the adult female having 2 pairs of legs; the male having a lateral spur on tibia I; and by the larviform female having 3 pairs of setae on the propodosoma. *C. macfarlanei* adult female has 1 pair of legs; the male has a spine on tibia I; and the larviform female has 2 pairs of setae on the propodosoma.

Female (Fig. 1): Gnathosoma wider than long, strongly sclerotized. Palpi reduced, without apparent setae on basal segments. Chelicerae not visible.

Idiosoma.—Eggshaped, smooth; without setae, and yellowish in alcohol; 5 subequal anterolateral lobes; dorsoventrally flat.

Legs.—Two pairs; 1st pair with 5 segments;

Fig. 1.—*Coccipolipus epilachnae*, new species, venter of female.

as figured; distal segment terminated with dark sclerotized hook-shaped claw, 1 dorsolateral short spur, and 1 fingerlike process. Second pair reduced in length; femur with short anterolateral, simple seta; distal segment terminated with 2 strong, short spurs. Body 517 μ long by 440 μ wide.

Larviform Female (Fig. 2, 3): Gnathosoma spherical, wider than long. Cheliceral cone protruding. Chelicerae thin, hooked shaped; with wide base and short stylets, ending without apparent teeth. Palpi 2-segmented; distal segment with 1 ventral simple seta and 1 anterolateral short spur. One pair of long lateral simple setae; ventrally and adjacent to this pair of setae, a smaller pair of simple setae.

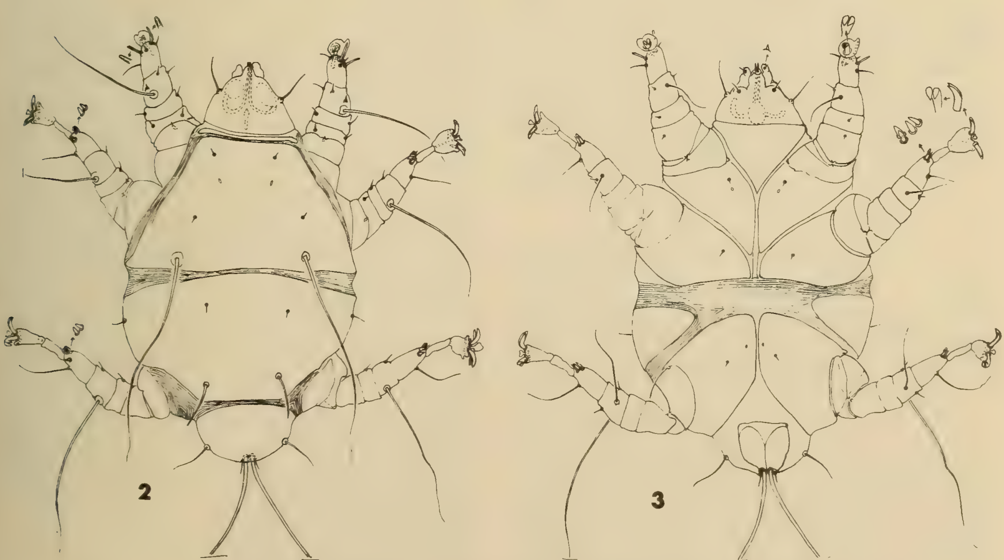
Dorsum.—Propodosomal shield rectangular shaped, wider than long; with 2 pairs of short subequal simple setae; 1 pair of pores; posterior pair of simple setae longer and stronger. Hysterosomal shield elongated; with 3 pairs of setae; humeral setae longer than anterior or posterior pair; posterior pair longer and stronger than anterior pair. Opisthosoma oval, with 1 pair of simple setae.

Venter.—Coxal plates I and II fused mesially, separated from plate III by fine striae. Each coxal plate with 1 seta; plate I and III each with small pore. One distinct plate on each side of body between legs II and III. Caudal plates well developed; with a pair of accessory setae, one on each side of caudal setae as figured.

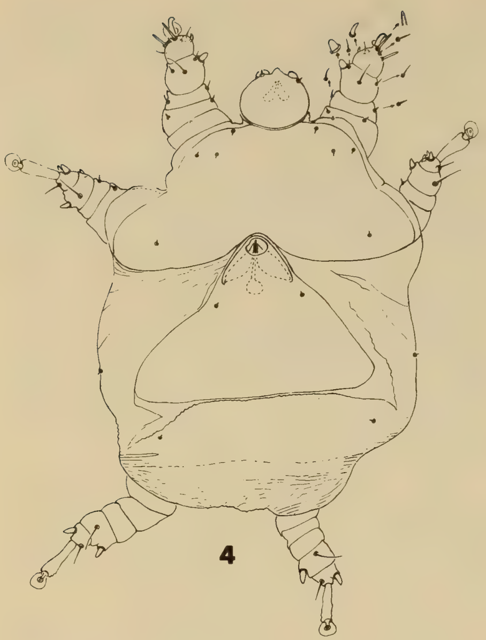
Legs.—Short and robust. Chaetotaxy on femur, genu, tibia and tarsus I: 2 + 1 spine−3−5+1 spine−2+1 spur+2 solenidia+1 bifurcate claw; leg II: 1 spur−1 spine−4−2+3 spurs+2 bifurcate claws; leg III: 0−1−4−2+3 spurs+2 bifurcate claws. Body 217 μ long by 127 μ wide.

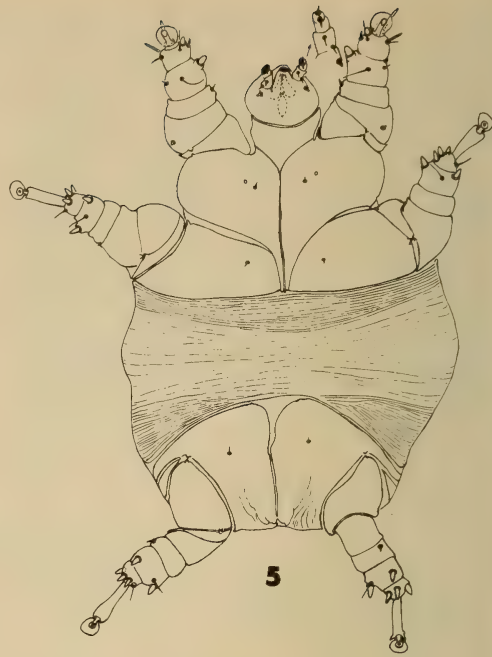
Male (Fig. 4, 5): Gnathosoma oval, wider than long. Cheliceral cone protruding. Chelicerae thick, wide at base; with short stylets, ending without apparent teeth or barbs. Palpi 3-segmented; distal segment with 1 microseta and 1 short spine; second segment without apparent setae; proximal segment with 2 microsetae; 1 pair of strong, short spurs adjacent to proximal segment; dorsal anterolateral margin with 1 pair short spurs, each spur located above palp as figured.

Dorsum.—Propodosomal shield elongated; with 4 pairs of microsetae as figured. Hysterosomal



Figs. 2, 3.—*Coccipolipus epilachnae*, new species, larviform female. 2, dorsum; 3, venter.





Figs. 4, 5.—*Coccipolipus epilachnae*, new species, male. 4, dorsum; 5, venter.

shield triangular; with 1 pair of microsetae; with aedeagus situated middorsally, the orifice at the apex of shield; with 1 pair of microsetae subequal in length in region of the metapodosoma.

Venter.—Pair coxal plates I and II fused mesially, coxal plates I and II each with 1 microseta; each coxal plate I with a pore. Coxal plates III without apparent pore, but with 1 pair of microsetae; metapodosoma separated from coxal plates II by fine striae. Caudal plates poorly developed.

Legs.—Short and robust. Chaetotaxy on femur, genu, tibia and tarsus **I**: 2+1 spur−2−4+1 thick thumb-like spur−2 solenidia+1 simple spur+1 thumb-like spur+3+1 uncinated claw; leg II: 1−1−2+2 spurs−1+3 spurs, claws absent; leg III: 0−1−2+2 spurs−1+3 spurs, claw absent. Body 159 μ long by 121 μ wide.

Holotype: Male, U. S. National Museum of Natural History No. 3620, collected from *Epilachna varivestis* Mulsant, originally found in San Salvador, El Salvador, 8 Dec. 1972, by Dr. F. F. Smith.

Paratypes: 3 females, 6 males and 32 larviform females with the above data.

Discussion: The preceding new mite species was made available for taxonomic study through the courtesy of Dr. Floyd F. Smith, Collaborator, Ornamentals Laboratory, ARS, USDA, Beltsville, Maryland. While on duty tour as Consulting Entomologist with the AID program in El Salvador, C.A., about 15 adult Mexican bean beetles, *Epilachna varivestis* Mulsant, were collected on 7 November 1971 from pole beans, taken from variety test plots at the National Agriculture Experiment Station at San Andres, El Salvador. By prearrangement, Dr. Roger Lawson was provided with a special permit, and he brought these beetles to the Beltsville Agricultural Research Center. They were delivered to Dr. W. W. Cantelo for conducting cross-mating studies with the local Mexican bean beetles. The El Salvador beetles in the colony were sluggish, fed little and laid few egg clusters. When the males were mated with virgin Beltsville females, the eggs produced were sterile, but Beltsville males mated with the virgin Salvador females resulted in the production of fertile eggs. These progeny were mated with El Salvador and Beltsville males and females in reciprocal crosses, and egg pro-

duction was normal (W. W. Cantelo, unpublished data).

On 20 July 1972, Dr. Smith and Ing. Jose Mancia, El Salvador Entomologist, collected a second lot of beetles that included adults and larvae from unstaked beans (27-R variety) in fields on the high slopes of the Volcano San Vincente. The beans were maturing and leaves were yellowing. They were scheduled for pulling and harvesting as dry beans in about 2 weeks. The foliage damage was estimated as less than 5%, very low by comparison with the usual damage in the U. S. However, it was the worst damage yet observed in El Salvador. No sprays had been applied for control. Dr. Smith brought samples of these beetles, under permit, to Beltsville on 24 July 1972. This colony was established in a separate cage. Again, adults fed little, rested on sides of the cages, and laid few eggs on the host plants. Fertile eggs were produced from all reciprocal crosses of males and virgin females of the Beltsville and El Salvador colonies. In late October egg production dropped in the colonies for no apparent reason. In November Dr. Boswell, Dr. Cantelo's assistant, observed slightly raised elytra and protruding bodies of small mites on some of the beetles. Upon raising the wing covers of adults of both colonies he found numerous mites closely packed together and apparently feeding on the dorsal surface of the host abdomen. Apparently these parasitic mites were associated with the decline in vigor and reproduction of the colonies. They probably gained access to the Beltsville colony when selecting individuals for cross mating tests with individuals from El Salvador. The colonies were not mixed in a common cage at any time. The mites may have been transferred on the hands or in vials used in handling the insects. A mite-free colony of bean beetles was established by examining a few egg clusters for absence of the mites and by rearing the hatching larvae in a separate green house. This colony with normal reproduction is now being maintained. Dr. Smith had assumed that the infested colonies would be retained for further experimentation to determine the potentialities of this new mite as a means of biological control for the Mexican bean beetle, but he failed to discuss this with anyone. Upon later inquiry he learned that after obtaining hatching larvae from isolated eggs, all other beetles were destroyed and cages thoroughly cleaned.

Although the parasitic mites were not discovered until after the mating tests had been made with beetles from the second collection from the San Vincente area, it is possible that they were present in the earlier collection from San Andres. Fewer cross mating tests were made from this colony, and any transfer of mites to beetles of the Beltsville colony was not evident. Since the beetles' behavior in both collections was similar, apparently, most if not all beetles from El Salvador were infested. All Mexican bean beetles had disappeared during the 1973 dry season, resulting in failure to collect mite-infested beetles for further studies at Beltsville.

Dr. Smith states, "from my 10 years observation in El Salvador during fairly regular periodic tours of duty with AID programs, the Mexican bean beetle was observed to be a minor pest in all bean growing seasons and required no insecticide treatment to protect the crops."

Studies are now being initiated to determine the mite's host range and potential effectiveness as a biological control agent.

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Beneficial Insect Introduction Laboratory, IIBIII, Beltsville, Maryland who collected additional specimens of the mite species here described from *Epilachna varivestis* and the mite *Coccipolipus macfarlanei* Husband from the coccinellid *Cycloneda sanguinea* (L.) at San Vincente, El Salvador on 17 July 1974.

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Biological Note on the Acridid Grasshopper Stenacris vitreipennis vitreipennis *(Marschall) (Insecta: Orthoptera)*

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ABSTRACT

Stenacris vitreipennis vitreipennis oviposits in the pithy stems of *Sagittaria* sp. in Florida.

All Leptysmini and many other Cyrtacanthacridinae are hygrophilous, frequenting vegetation growing in or about ponds, streams, and lakes, and occurring at times even on grasses and sedges standing in water of considerable depth. Biological information on *Stenacris* is scanty, but *Cornops aquaticum* Bruner, another cyrtacanthacridine, is known to oviposit in the thick soft petioles of the leaves of a water hyacinth, *Eichhornia azurea* (Swartz), a common plant in the streams and rivers of Uruguay (de Zolessi 1956).

Rehn (1952) referred to *Gesonula punctifrons* (Stal) ovipositing in the succulent stems of taro. Rehn and Hebard (unpublished information in Rehn and Eades 1961) noted *Stenacris vitreipennis vitreipennis* on arrowhead, *Sagittaria* sp., at Tallahassee, Florida, but did not record any information on the biology. This species was reared from egg-pods deposited in the pithy stems of *Sagittaria* sp. at a pond 1 mi north of Spring Creek, Wakulla Co., Florida in the summer of 1973. Egg-pods were inserted in the stems of the *Sagittaria*, and recovered from below water level, although they were not necessarily deposited below water level because there were marked fluctuations in water level in the pond both prior to and following the discovery of the oviposition scars and embedded egg-pods.

The hatching of the nymphs in the laboratory corresponded with collection of first-instar nymphs in the field. Subsequent collections resulted in the collection of nymphs and adults in the late spring and summer of 1973, but details of the occurrence of the various instars were not noted. Preserved material of the

nymphs is deposited in the collections of the Laboratory for Aquatic Entomology, Florida Agricultural and Mechanical University, Tallahassee, Florida. Specimens of the eupelmid egg parasite and parasitised egg pods are deposited, together with adult grasshoppers, in the United States National Museum.

Acknowledgments

I am grateful to Dr. Ashley B. Gurney, Systematic Entomology Laboratory, ARS, USDA, for references to literature dealing with this unusual mode of oviposition in acridoid grasshoppers.

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Daktulosphaira vitifoliae *(Fitch), the Correct Name of the Grape Phylloxeran (Hemiptera: Homoptera: Phylloxeridae)*

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ABSTRACT

Daktulosphaira vitifoliae (Fitch) is shown to be the correct name of the grape phylloxeran. The numerous name combinations by which the insect has been known, the synonyms, and the various spellings of its generic and specific names are listed.

An investigation was undertaken to determine the correct name of the grape phylloxeran. This action was desirable because more than one spelling of the specific name and more than one name combination are in current use for the species. The inquiry revealed that *Daktulosphaira vitifoliae* (Fitch) is the oldest available name for the species.

The grape phylloxeran, a native of North America, has been of economic importance since its accidental introduction into Europe and other viticultural centers of the world in the last century. At that time it virtually destroyed the grape industry in severely infested areas. Although the insect is no longer seriously destructive in some areas, it is injurious in others, and its symbionts, its biology, and its control are being studied. Federov (1959) discussed the injuriousness of the phylloxeran and stressed the need for its adequate control. Shaposhnikov (1967) stated, "This is the most serious pest of grapevine." Maillet (1957) gave an extensive discussion, review, and bibliography of the species. Literature on the biology, morphology, ravages, and control of *vitifoliae* is voluminous.

The names, spellings, accreditation of author names and the earliest noted publication of names of the grape phylloxeran are as follows:

Pemphigus Vitifoliae Fitch 1855: 862.
Byrsocrypta? (pemphigus) vitifoliae (Fitch).—Walsh 1863: 305.

Pemphigus vitifolia Fitch.—Shimer 1866: 290.
Daktulosphaira vitifoliae (Fitch).—Shimer 1866: 365.
Dactylosphaera? vitifoliae (Fitch).—Shimer 1867: 2.
Viteus vitifoliae (Fitch).—Shimer 1867: 6.
Rhizaphis vastatrix Planchon (*in* Bazille, Planchon and Sahut) 1868: 336.
Rizaphis vastatrix Planchon (*in* Bazille, Planchon and Sahut) 1868: 336.
Phylloxera vastatrix (Planchon) 1868: 588.
Rhyzaphis vastatrix (Planchon).—Signoret 1869: 580.
Pemphigus vitis folii (Fitch).—[Planchon and Lichtenstein] 1869: 189.
Phylloxera vitifoliae (Fitch).—Walsh and Riley 1869: 248.
Pemphigus vitifolii (Fitch).—Signoret 1869: 565.
Peritymbia vitisana Westwood 1869: 109.
Phylloxera vitis folii (Fitch).—Planchon and Lichtenstein 1871: 5.
Rhizovaga devastatrix Hartig 1879: 269.
Dactylosphaera vitifolii (Shimer).—Lichtenstein 1885: 44.
Perytimbia vastatrix (Planchon).—Lichtenstein 1885: 161.
Phylloxera pemphigoides Donnadieu 1887: 1246.
Rhizocera vastatrix (Planchon).—Kirk 1897: 3.
Xerampelus vastator (Planchon).—Del Guercio 1900: 80.
Peritymbia vitifolii (Fitch-Riley).—Börner 1908: 601.
Phylloxera vitifolii (Fitch-Riley).—Börner 1908: 609.
Peritymbia vitifolii pervastatrix Börner 1910: [4].
Phylloxera (Viteus) vastator (Planchon).—Grassi 1912: 10.
Phylloxera (Viteus) vastatrix (Planchon).—Grassi 1912: 10.
Peritymbia vitisfoliae (Fitch).—Grassi 1912: 10.
Peritymbia vitifolii (Fitch).—Grassi 1912: 10.
Peritymbia vitisfolii (Fitch).—Grassi 1912: 10.
Viteus vitisfolii (Fitch).—Grassi 1912: 10.
Phylloxera (Peritymbia) pervastatrix Börner 1914: 219.
Peritymbia (Phylloxera) vitifolii pervastatrix Börner 1914: 59.
Daktulosphaira (Pemphigus) vitifoliae (Fitch).—Börner 1930: 159.
Pemphigus (Viteus) vitifoliae (Shimer).—Börner and Schilder 1932: 698.
Viteus vitifolii (Fitch).—Börner 1952: 212.
Viteus vitifolii vulpinae Börner 1952: 213.
Dactylosphaera (Peritimbia) vitifolii (Fitch).—Ambrus 1959: 526.

Although *vastatrix* was used extensively for several years after publication, it as well as the other specific names listed above have long been recognized as synonyms of *vitifoliae*.

Fitch (1855: 862, 1855: 158, 1857: 397) invariably called the insect whose galls he observed on grape leaves in New York State "the grape leaf louse (*Pemphigus Vitifoliae*)." Signoret (1869: 556, 565) spelled the name *vitifolii*. Planchon and Lichtenstein (1871: 5) stated that *vitifoliae* was incorrect and should be rectified to *vitis folii*. Riley (1871: 95) rejected their opinion, stating ". . . though "*folii*" would of course be more grammatically correct, one would suppose the Doctor [Fitch] had some reason for his conduct." Thomas (1879: 158) also indicated that the spelling should be *vitis-folii* or *vitifolii*, but he approved *vitifoliae*, and wrote ". . . names with the termination have been too long received for this to be a valid objection in this case." Grassi (1912: 10) suggested *vitisfoliae* as well as *vitisfolii* and *vitifolii*. All spellings except *vitisfoliae* have been used, with Europeans tending to use *vitifolii* and Americans usually using *vitifoliae*.

Article 32(a)(ii) of the International Code of Zoological Nomenclature states " . . . incorrect transliteration, improper latinization, and use of an inappropriate connecting vowel are not to be considered inadvertent errors. . ."

Thus according to the Code, *vitifoliae* is the legal spelling of the name.

The generic name with which *vitifoliae* has been combined has varied. Fitch (1855, 1857) always, and Walsh (1863) originally, placed *vitifoliae* in aphid genera. Later Walsh (1866: 111, 1867: 284) indicated that Fitch erred in considering the insect an aphid, did not mention his own 1863 assignment, and stated (1867: 284) that the insect was ". . . a true bark-louse belonging to the *Coccus* family" and that it ". . . must become the type of a new and very aberrant genus."

Shimer (1866: 290) studied "*Pemphigus vitifolia*" stating, "The result of these investigations developes a new genus, of a new family, in the third division *Monomera* of the *Homoptera*, for this and another insect (also one of Mr. Walsh's coccus) found in a small subglobular gall on the leaf of the Pignut Hickory; and probably, some two or three other insects that I have seen. These may possibly comprehend more than one genus when more thoroughly studied."

Shimer (1866: 290) then described but did not name the genus, indicated that *vitifoliae* and possibly another species belonged in it, and stated, "The insect inhabiting the small gall on the Pignut Hickory (*Caoja*[!] *glabra*) and which doubtless is identical with that referred to by Mr. Walsh, P.E., 111, although the galls are mostly all larger than a "cabbage seed," I believed after careful examination of the female and larva to belong to the same genus as the "grape leaf louse," and suggested for it the species name of *globosum*."

The reference to Walsh and the size of the gall were the only statements that could be construed as a description of *globosum*. Walsh (1866: 111–112) described the gall referred to by Shimer as ". . . an undescribed gall the size of a cabbage-seed on the leaves of the Pignut Hickory (*Carya glabra*)." This presumably meets the requirements of Article 16(a) (viii) of the Code as an indication of a specific name, but it does not meet the requirements of Article 11(g) (ii) because *globosum* was not combined with a generic name.

Shimer (1866: 365) referred to his earlier (1866: 290) article, named and briefly described the new genus *Daktulosphaira* and placed a single species, *Pemphigus vitifoliae* Fitch, in it but (p. 365) did not mention *globosum*. And he did not give the derivation of his new generic name which is, according to Steyskal (1974), a literal translation of the Greek.

The following year Shimer (1867: 2) described "Dactylosphaera. New genus" and gave the Greek from which the name was derived. This spelling, also according to Steyskal (1974), is a classical Latin transcription of the name. Shimer (1867: 2–11) also described "Dactylosphaera globosum, n. sp." placing that species before *vitifoliae* which he assigned to *Dactylosphaera* with a question. His only mention of his former articles was to state that in 1866 he had called *vitifoliae* the "Grape leaf louse." He (1867: 5–8) redescribed *vitifoliae* and stated "In case, however, the characters given above should be sufficient to separate, generically, *vitifoliae* from *D. globosum*, I would propose the generic name of *Viteus* for the former."

Walsh (1867: 24–28) immediately accepted *Dactylosphaera vitifoliae* as the correct name for the grape leaf louse, while Riley (1871: 84) used *Phylloxera vitifoliae*, the name that was used much more frequently than *Daktulosphaira vitifoliae* or *Dactylosphaera vitifoliae* for many years.

Because Shimer's original descriptions of *globosum* and *Daktulosphaira* were published in a farm journal of uncertain distribution, and because the articles were not cited in his 1867 publication where he described *globosum* and *Dactylosphaera* as new, the 1866 articles presumably were overlooked or ignored by some workers while others apparently assumed that *Dactylosphaera* was a correction of *Daktulosphaira* and that the

name should date from 1867. Later Shimer (1869: 386–398) described or redescribed several phylloxeran species that lived in galls on hickories, placing them in *Dactylosphaera*. He (p. 392–393) again mentioned *D. globosum* as a species living on hickories but did not use the name *vitifoliae*.

Pergande (1904: 236b–238) treated *globosum* as *Phylloxera globosum*, citing Shimer's 1867 publication. Although Pergande did not discuss the status of *Dactylosphaera*, he presumably considered the name a synonym of *Phylloxera*, because he placed in the latter genus the various species included in *Dactylosphaera* by Shimer in 1869. Pergande (p. 213) mentioned *vitifoliae* only in a quotation from Shimer 1867 and did not refer to Shimer's 1866 articles.

Wilson (1910: 150, 155) listed *Dactylosphaera* Shimer 1867 with *globosum* the type, *Daktulosphaira* Shimer 1866 with *vitifoliae* the type, and *Viteus* Shimer 1867 also with *vitifoliae* the type. Börner (1930: 159, 162, 193) synonymized *Viteus* with *Daktulosphaira*, but later (1952: 212, 227) recognized as valid genera, *Dactylosphaera* with *globosum* as its type-species and *Viteus* with *vitifoliae* as its type-species, stating that *Daktulosphaira* (1866) with *vitifoliae* as its type was an error for *Dactylosphaera* (1867).

Prior to 1952, both while and after synonymous names were in use, *Phylloxera vitifoliae* was the most commonly used name for the species. *Dactylosphaera vitifoliae* appeared occasionally, and *Daktulosphaira vitifoliae* and *Viteus vitifoliae* were rarely cited. Since 1952 non-Americans have tended to use *Dactylosphaera vitifoliae* (or *vitifolii*) or *Viteus vitifoliae* (or *vitifolii*) while Americans, without critical consideration of the insect's name or relationships, have continued the use of *Phylloxera vitifoliae*.

Daktulosphaira falls under Article 32(a)(ii) of the Code because, in the original publication, there is no "clear evidence of an inadvertent error, such as a lapsus calami, or a copyist's or printer's error" and "(incorrect transliteration, improper latinization. . . . are not to be considered inadvertent errors)." Since *vitifoliae* was the only species included in the genus, *Daktulosphaira vitifoliae* (Fitch) is the correct name for the grape phylloxeran, and *Viteus* is a synonym of the older generic name. Because there has not been unanimous use of one name in recent years, common usage would not be seriously disrupted by using *Daktulosphaira vitifoliae*, the name that merits general acceptance.

The identity of *globosa* is uncertain. Types of the species are not known to exist, and morphological characteristics of the insects have not been adequately diagnosed. Recognition of the species has depended primarily on the appearance of its galls which Shimer (1869: 392–393) indicated he believed he had confused with galls of *Dactylosphaera caryaesemen* Walsh in his 1867 description of *globosum*. Pergande (1904: 213, 237) affirmed this opinion and redescribed and illustrated the galls of the two species. Shimer (1869: 393) also indicated that the trees on which he observed *globosum* and *caryaesemen* in 1867 were *Carya amara* instead of *Carya glabra* as he had previously reported. Pergande (1904: 213) noted galls of *caryaesemen* on *Carya glabra* in the Mississippi River Valley but did not state whether galls of *globosum* were present. I believe there may be some uncertainty concerning the true host(s) of *globosa*.

Perhaps it would be possible to collect galls and specimens of *globosa* and, after critical field and laboratory studies, determine the identity of the species. But until such studies are made, the status of *globosa* and *Dactylosphaera* will remain unclear.

Daktulosphaira vitifoliae differs morphologically and biologically from *Phylloxera quercus* Boyer de Fonscolombe (1834: 223–224), the type-species of *Phylloxera* Boyer de Fonscolombe (1834: 222), and the two are not congeneric. *D. vitifoliae* lacks prominent, tuberculate dorsal and marginal proc-

esses and lives in galls on the leaves and in cavities of swellings on the roots of *Vitis*. *P. quercus* has strongly developed, elongate processes on the dorsum and margin of the body in apterae and on the head and thorax in alatae. This species lives on the lower surface of the leaves of oak and does not cause galls.

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Synonymical Notes on Larrinae and Astatinae (Hymenoptera: Sphecidae)

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ABSTRACT

The following new synonyms, new combinations, and new names are indicated: *Tachysphex projectus* Nurse, 1905, and *Tachysphex rufoniger* Bingham, 1897 = *Tachysphex pompiliformis* (Panzer), 1805; *Tachysphex mysticus* Pulawski, 1971 = *Tachysphex excelsus* Turner, 1917; *Tachysphex latissimus* Turner, 1917, and *Tachysphex pectoralis* Pulawski, 1964 = *Tachysphex erythrophorus* Dalla Torre, 1897; *Tachysphex laniger* Pulawski, 1964 = *Tachysphex gujaraticus* Nurse, 1909; *Tachysphex japonicus* Iwata, 1933 = *Tachysphex nigricolor* (Dalla Torre, 1897); *Tachysphex varihirtus* Cameron, 1903, *Tachysphex rugidorsatus* Turner, 1915, and *Tachysphex mindorensis* Williams, 1928 = *Tachysphex puncticeps* Cameron, 1903; *Tachysphex spinosus* Pulawski, 1974, nec Fox, 1893 = *Tachysphex spinulosus* Pulawski, new name; *Tachysphex brevitarsis* Kohl, 1901 = *Tachysphex bengalensis* Cameron, 1889; *Tachytes sericops* Smith, 1856, *Tachysphex depressus* (Saussure), 1867, and *Tachysphex helmsi* (Cameron), 1888 = *Tachysphex nigerrimus* (Smith), 1856; *Tachysphex lilliputianus* Turner, 1917 = *Tachysphex minutus* Nurse, 1909; *Tachysphex imperfectus* de Beaumont, 1940 = *Tachysphex fulvicornis* Turner, 1918; *Tachytes ceylonicus* Cameron, 1900, and *Tachytes aurifrons* Cameron, 1900, nec Lucas, 1849 = *Tachysphex panzeri* (vander Linden), 1829; *Tachysphex ablatus* Nurse, 1909 = *Tachysphex panzeri pulverosus* (Radoszkowski), 1886; *Tachysphex foucauldi* de Beaumont, 1952 = *Tachysphex vulneratus foucá uldi* de Beaumont; *Tachysphex heliophilus* Nurse, 1909 = *Tachysphex schmiedeknechti* Kohl, 1883; *Tachysphex strigatus* Turner, 1917 = *Tachysphex subfuscatus* Turner, 1917; *Tachysphex inventus* Nurse, 1903 = *Tachysphex erythropus*

(Spinola), 1838; *Tachysphex actaeon* de Beaumont, 1960 = *Tachysphex selectus* Nurse, 1909; *Tachysphex fluctuatus* (Gerstaecker), 1857 = *Tachysphex sericeus* (Smith), 1856; *Tachysphex pollux* Nurse, 1903 = *Holotachysphex holognathus* (Morice), 1897; *Prosopigastra acanthophora* Gussakovskij, 1933 = *Prosopigastra creon* (Nurse), 1903; *Homogambrus cimicivorus* Ferton, 1912 = *Prosopigastra creon cimicivora* (Ferton); *Prosopigastra carinata* Arnold, 1922 = *Prosopigastra creon carinata* Arnold; *Tachysphex nudus* Nurse, 1903 = *Prosopigastra nuda* (Nurse); *Parapiagetia integra* (Kohl), 1892 = *Parapiagetia genicularis* (F. Morawitz), 1890; *Tachysphex substriatulus* Turner, 1917 = *Parapiagetia substriatula* (Turner); *Parapiagetia denticulata* (Morice), 1897, and *Parapiagetia saharica* de Beaumont, 1956 = *Parapiagetia erythropoda* (Cameron), 1889; *Larrada obliqua* Smith, 1856 = *Larropsis (Ancistromma) obliqua* (Smith); *Tachytes serapis* Pulawski, 1962 = *Tachytes fucatus* Arnold, 1951; *Tachytes maculitarsis* Cameron, 1900 = *Tachytes brevipennis* Cameron, 1900; *Tachytes griseolus* Arnold, 1951, and *Tachytes rufitibialis* Arnold, 1951 = *Tachytes diversicornis* Turner, 1918; *Tachytes pulchricornis kolaensis* Turner, 1917 = *Tachytes pulchricornis* Turner, 1917; *Tachytes patrizii* Guiglia, 1932 = *Tachytes comberi* Turner, 1917; *Tachytes andreniformis* Cameron, 1902, nec Cameron, 1889, and *Tachytes fulvovestitus* Cameron, 1904 = *Tachytes fulvopilosus* Cameron, 1904; *Tachytes proximus* Nurse, 1903 = *Tachytes tabrobanae* Cameron, 1900; *Tachytes maculipennis* Cameron, 1904 = *Tachytes modestus* Smith, 1856; *Tachytes shiva* Nurse, 1903, *Tachytes varipilosus* Cameron, 1905, and *Tachytes formosanus* Tsuneki, 1966 = *Tachytes saundersii* Bingham, 1897; *Tachytes suluensis* Williams, 1928 = *Tachytes saundersii suluensis* Williams; *Tachytes guichardi* Arnold, 1951 = *Tachytes basilicus* Guérin-Méneville, 1844; *Tachytes neavei* Turner, 1917 = *Tachytes velox* Smith, 1856; *Tachytes melanopygus* Costa, 1893 = *Tachytes argyreus* (Smith), 1856; *Tachytes basalis* Cameron, 1889, *Tachytes calvus* Turner, 1929, and *Tachytes seminudus* Arnold, 1951 = *Tachytes pygmaeus* Kohl, 1888; *Gastrosericus binghami* Cameron, 1897 = *Gastrosericus rothneyi* Cameron, 1889; *Liris nitidus* Cameron, 1913 = *Liris aurifrons* (Smith), 1859; *Notogonia pseudoliris* Turner, 1913 = *Liris croesus* (Smith), 1856; *Notogonia chapmani* Cameron, 1900 = *Liris jaculator* (Smith), 1856; *Notogonia pulchripennis* Cameron, 1889, and *Notogonia luteipennis* Cameron, 1890 = *Liris conspicua* (Smith), 1856; *Astata argenteofascialis* Cameron, 1889 = *Lyroda argenteofacialis* (Cameron); *Odontolarra rufiventris* Cameron, 1900 = *Lyroda formosa* (Smith), 1859; *Astata agilis* Smith, 1875 = *Astata boops* (Schrank), 1781; *Astata chilensis* Saussure, 1854 = *Astata australasiae* Shuckard, 1837; *Astata stecki* de Beaumont, 1942 = *Astata kashmirensis* Nurse, 1909; *Astata absoluta* Nurse, 1909 = *Astata compta* Nurse, 1909; *Astata fletcheri* Turner, 1917, and *Astata hirsutula* Gussakovskij, 1933 = *Astata quettae* Nurse, 1903; *Astata eremita* Pulawski, 1959 = *Astata lubricata* Nurse, 1903; *Astata interstitialis* Cameron, 1907 = *Astata (Dryudella) orientalis* Smith, 1856. A number of lectotypes are designated for the first time.

During the last few years I have had several opportunities to examine types of Sphecidae preserved in various European institutions, and my recent stay in the British Museum (Natural History) was dedicated mainly to studying types. As a result, numerous new synonyms and new combinations have been found, and they are discussed below.

There are several reasons why a species may be described 2 or more times as new: inaccurate diagnoses of previous writers, insufficient knowledge of taxonomically important characters, absence of good diagnostic features, sexual dimorphism, scarcity of material, etc. A usually unappreciated factor is the occurrence of many species in 2 or 3 zoogeographic regions; in such cases they often receive a different name in each region. Indeed, authors dealing with the Palaearctic, Ethiopian and Oriental Larrinae and Astatinae (including myself), usually have concentrated on the fauna of 1 region and ignored species inhabiting other territories. The number of synonyms resulting from such limited surveys is considerable, and several examples are found below.

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In the following text, an exclamation mark before the name of a species indicates that the holotype or syntypes have been examined.

Tachysphex pompiliformis (Panzer)

!*Larra pompiliformis* Panzer, 1805: pl. 13, ♀. Holotype ♀: Germany (Zool. Samml. Munich).
!*Tachysphex projectus* Nurse, 1903b: 517, ♀. Holotype ♀: Kashmir, 5000–6000 ft. (British Mus., Type Hym. 21.244). **Syn. n.**
!*Tachysphex rufo-niger* Bingham, 1897: 195, ♀. Lectotype ♀: "North-West Provinces [of India],"? or Pakistan (British Mus., Type Hym. 21.247), present designation. **Syn. n.**

The types of *T. projectus* and *T. rufoniger* are identical with the cómmon *T. pompiliformis*.

Tachysphex excelsus Turner

!*Tachysphex excelsus* Turner, 1917a: 320, ♀. Lectotype ♀: China: Tibet: Gyangtse, 13000 ft. (British Mus., Type Hym. 21.266). **Present designation.**
!*Tachysphex mysticus* Pulawski, 1971: 81, ♀, ♂. Holotype ♀: Mongolia: Ulan-Bator (Zool. Inst. Leningrad). **Syn. n.**

The 3 syntype females of *T. excelsus* possess the diagnostic characters of *T. mysticus*: peculiar clypeus (highest point of middle clypeal lobe near clypeal hindmargin); vertex hair erect; gastral terga without silvery, apical fasciae; terga I-III with sparse, microscopic punctures; underside of forefemora with fine, dense, punctures, and with large, sparse, punctures; outer face of foretibiae with glabrous zone. The disk of the mesoscutum is sparsely punctate (punctures several diameters apart).

Tachysphex erythrophorus Dalla Torre

!*Tachysphex erythrogaster* Cameron, 1889: 143, ♀. Holotype ♀: India: Bombay State: Poona (Oxford Univ. Mus., coll. Rothney). *Nec Tachysphex erythrogaster* (Costa), 1882.
Tachysphex erythrophorus Dalla Torre, 1897: 679 (new name for *T. erythrogaster* Cameron, nec Costa).
!*Tachysphex latissimus* Turner, 1917d: 199, ♀, ♂. Lectotype ♀: India: Bihar: Pusa (British Mus., Type Hym. 21.254). **Present designation; syn. n.**
!*Tachysphex pectoralis* Pulawski, 1964: 101, ♀, ♂. Holotype ♀: Egypt: Abu Rawash NW of Cairo (coll. Pulawski). **Syn. n.**

The types of *T. erythrogaster* Cameron and *T. latissimus* agree with *T. pectoralis*, as characterized by me (Pulawski, 1971). Diagnostic characters are: vestiture long, woolly, completely obscuring integument of mesopleura and forecorners of mesoscutum; anterior (oblique) part of mesosternum with short, suberect hair; legs and usually gaster red; flagellomere I long. The holotype of *T. erythrogaster* Cameron bears a label "*Larra erythrogaster* Cameron." The lip of its clypeus is distinctly emarginate mesally. Unlike all other specimens seen, gastral terga II-V are black (except for colourless apical depressions) in the lectotype of *T. latissimus*.

Tachysphex gujaraticus Nurse

!*Tachysphex gujaraticus* Nurse, 1909: 516, ♀, ♂. Lectotype ♀: India: Bombay State: Deesa, 140 km NNE of Ahmadabad (British Mus., Type Hym. 21.247). **Present designation.**
!*Tachysphex laniger* Pulawski, 1964: 105, ♀, ♂. Holotype ♀: Egypt: Abu Rawash NW of Cairo (coll. Pulawski). **Syn. n.**

The types of *T. gujaraticus* and *T. laniger* are identical. Diagnostic characters of *T. gujaracticus* are: vestiture long, woolly, almost totally hiding sculpture of mesopleura and forecorners of mesoscutum; gaster and legs red in female and usually in male; lip of female clypeus denticulate.

Tachysphex nigricolor (Dalla Torre), **comb. n.**

!*Larrada nigricans* Smith, 1873: 192 (sex not mentioned). Lectotype ♀: Japan: Nagasaki (British Mus., Type Hym. 21.256. **Present designation.** *Nec* Walker, 1871.
Larra nigricolor Dalla Torre, 1897: 670 (new name for *Larrada nigricans* Smith, nec Walker).

Tachysphex japonicus Iwata, 1933: 47, ♀, ♂. Holotype ♀: Japan: Honshu: Ikeda, Settsu (Kyoto Univ. or Tokyo Mus.). Syn. n.

Tsuneki (1964a) synonymized *Larra nigricans* Smith with *Liris japonica* Kohl, but the lectotype female of the former species is actually identical with *T. japonicus* Iwata (the only representative of the genus in Japan). For the characteristics of this species see Pulawski (1971) and Tsuneki (1971).

Tachysphex puncticeps Cameron

!*Tachysphex puncticeps* Cameron, 1903: 127, ♀. Holotype ♀: India: Bengal: Barrackpore, 20 km N of Calcutta (Oxford Univ. Mus., coll. Rothney).
!*Tachysphex varihirta* Cameron, 1903: 128, ♂. Holotype ♂: India: Bengal: Barrackpore (Oxford Univ. Mus., coll. Rothney). **Syn. n.**
!*Tachysphex rugidorsatus* Turner, 1915: 556, ♀. Lectotype ♀: Tasmania: Eaglehawk Neck (British Mus., Type Hym. 21.216). **Present designation; syn. n.**
!*Tachysphex mindorensis* Williams, 1928: 92, ♂, ♀. Holotype ♂: Philippines: Mindoro Is.: San Jose (Bishop Mus. Honolulu). **Syn. n.**

This species belongs to the *pompiliformis* group and is rather similar to *T. nitidus* Spinola. It may be easily recognized by the mesoscutal hair which is shorter than the midocellus diameter.

Tachysphex spinulosus, nom. n.

!*Tachysphex spinosus* Pulawski, 1974: 72, ♀. Holotype ♀: Brazil: Mato Grosso: Xavantina (British Mus.). *Nec* W. Fox, 1893.

Tachysphex bengalensis Cameron

!*Tachysphex bengalensis* Cameron, 1889: 154, ♀. Lectotype ♀: India: "Bengal: Tirhoot" = Bihar: Muzalfarpur (Oxford Univ. Mus., coll. Rothney). **Present designation.**
!*Tachysphex brevitarsis* Kohl, 1901: 783, ♀. Syntypes: Ceylon: Badurelia (Nathist. Mus. Vienna). **Syn. n.**

T. bengalensis is probably a roach collector because tarsomeres IV of the female are obtusely emarginate and wider than long, a trait common to such wasps. It differs from other Indian *Tachysphex* by the long, woolly hair on the head and thorax, strongly punctate mesopleura, ridged propodeal sides, and black body.

The species called *T. bengalensis* by Williams (1928) is actually *T. tinctipennis* Cameron (holotype seen). It is very different from true *T. bengalensis* but is rather similar to *T. nigricolor*.

Tachysphex nigerrimus (Smith)

!*Tachytes nigerrimus* Smith, 1856: 302, ♀ (reference to White's MS name *Larra nigerrima*). Holotype ♀: New Zealand (British Mus., Type Hym. 21.213). White in Butler, 1874: pl. 7 (*Astata*); Turner, 1908: 491 (*Tachysphex*).
!*Tachytes sericops* Smith, 1856: 302, "♀" = ♂. Holotype ♂: New Zealand (British Mus., Type Hym. 21.212). **Syn. n.**
!*Tachytes depressus* Saussure, 1867: 69, ♀. Holotype ♀: New Zealand (Nathist. Mus. Vienna). **Syn. n.** Kohl, 1885: 401 *(Tachysphex).*
Tachytes Helmsii Cameron, 1888: 182, ♀. Type(s): New Zealand: Greymouth (location unknown). **Syn. n.** Kirkaldy, 1910: 131 *(Tachysphex).*

The types of *T. nigerrimus* and *T. sericops* are the opposite sexes of one species. I was unable to locate the type of *T. helmsii*, but this species is certainly a synonym of *T. nigerrimus*. In the original description, Cameron says "this species belongs to the genus *Tachysphex* (Kohl)," and the only *Tachysphex* occuring in New Zealand is *T. nigerrimus*. A female specimen in the British Museum bears a label in Cameron's handwriting "*Tachytes Helmsi* Cam. N. Zealand," and it is *T. nigerrimus*; obviously this is the specimen mentioned by Cameron (1898).

Diagnostic characters of this species are: female mandibles without lobe midway on lower margin; female clypeus almost flat, with middle lobe slightly raised along mid line; male hindtibiae fusiform, narrow basally, male foretarsi with rake.

Tachysphex minutus Nurse

!*Tachysphex minutus* Nurse, 1909: 516, ♀, ♂. Lectotype ♂: India: Bombay State: Deesa (British Mus., Type Hym. 21.258a). **Present designation.**
!*Tachysphex lilliputianus* Turner, 1917d: 198, ♂. Holotype ♂: India: Bihar: Pusa (British Mus., Type Hym. 21.254). **Syn. n.**

This species belongs to the *brevipennis* group and is very similar to *T. rugosus* Guss. and *T. quadrifurci* Pul. It differs from the former by the longer

foretarsal rake in the male and from the latter by the female gena, which is thick as seen from above. Possibly these differences represent geographic variation.

Tachysphex fulvicornis Turner

!*Tachysphex fulvicornis* Turner, 1918b: 363, ♀. Holotype ♀: India: Bengal: Chapra (British Mus., Type Hym. 21.259).
Tachysphex imperfectus de Beaumont, 1940: 178, ♀. Holotype ♀ (de Beaumont, 1947a: 210): Algeria: Biskra (Oxford Univ. Mus.). **Syn. n.**

The holotype of *T. fulvicornis* is identical with North African specimens of *T. imperfectus* in all taxonomically important characters, such as rugose frons, very densely punctate gastral terga, basal flagellomeres ferrugineous, etc., but it differs from other females in having a tectiform pygidial area, a rather unimportant character.

Tachysphex panzeri (vander Linden)

!*Tachytes panzeri* vander Linden, 1829: 22, ♂, ♀ (♀ = *Tachysphex pseudopanzeri* de Beaumont). Type: Spain (lost). Neotype ♂ (Pulawski, 1971: 262): Spain: Toledo (Rijksmus. Nat. Hist. Leiden). Kohl, 1884: 368 (*Tachysphex*).
!*Tachytes ceylonica* Cameron, 1900a: 21, ♂. Holotype ♂: Ceylon (Oxford Univ. Mus.). **Syn. n.**
!*Tachytes aurifrons* Cameron, 1900a: 23, "♀" = ♂. Lectotype ♂: Ceylon: Trincomalli (British Mus., Type Hym. 21.252). **Present designation**. *Nec Tachytes aurifrons* Lucas, 1848 = *Tachysphex panzeri* (vander Linden). **Syn. n.**

The types of *T. aurifrons* Cameron and *T. ceylonicus* do not differ from the Palaearctic *T. panzeri*. All have the same external morphology, and the shape of the volsella is identical.

Tachysphex panzeri pulverosus (Radoszkowski)

!*Tachytes pulverosus* Radoszkowski, 1886: 32, ♀, ♂. Syntypes: Uzbek SSR: Samarkand (Zool. Inst. Cracow).
!*Tachysphex ablatus* Nurse, 1909: 516, ♀. Lectotype ♀: India: Bombay State: Deesa (British Mus., Type Hym. 21.253), present designation. **Syn. n.**

I could not find any difference between the lectotype female of *T. ablatus* and a female of *T. panzeri pulverosus* from Fezzan, Libya, with which I compared it.

Tachysphex vulneratus foucauldi de Beaumont, **stat. n.**

!*Tachysphex vulneratus* Turner, 1917b: 325, ♀. Lectotype ♀: East Zambia: Niamadzi River near Nawalia, 2000 ft. (British Mus., Type Hym. 21.214). **Present designation.**
!*Tachysphex foucauldi* de Beaumont, 1952b: 190, ♂. Holotype ♂: Algeria: Hoggar: Tinhamour (Mus. Zool. Lausanne).

T. vulneratus and *T. foucauldi* are certainly conspecific, but they differ enough to be recognized as geographic races. In the nominate subspecies, the gaster is red apically, and the tibiae are pubescent throughout; in the female, the gena is moderately developed as seen from above, the mesoscutal interspaces are mat, the mesopleural interspaces are linear, tergum V is finely, densely punctate, the undersides of the fore and midfemora are covered with appressed hair; and in the male, the dorsal, apical process of the volsella is widely rounded.

In *T. vulneratus foucauldi* the gaster is black, and the outer face of the foretibia is glabrous; in the female, the gena is narrow as seen from above, the mesoscutal interspaces are shining, the mesopleura are rugose (punctures contiguous), tergum V is finely, sparsely punctate, and the undersides of all femora are glabrous; in the male, the dorsal, preapical process of the volsella is narrow, truncate.

Tachysphex schmiedeknechti Kohl

!*Tachysphex Schmiedeknechti* Kohl, 1883: 170, ♀. Syntypes: Greece: Egina = Aiyina (Nathist. Mus. Vienna).
!*Tachysphex heliophilus* Nurse, 1909: 515, ♀, ♂. Lectotype ♀: India: Bombay State: Deesa (British Mus., Type Hym. 21.245). **Present designation; syn. n.**

T. schmiedeknechti may be easily recognized by the peculiar, reticulate sculpture of the mesoscutum and mesopleura, by the densely serrate inner hindtibial spur, and by the dark, trans-

verse band of the forewing. Usually the female gaster is black in this species, but in the lectotype female of *T. heliophilus* tergum I and II are brownish red with black spots.

Tachysphex subfuscatus Turner

!*Tachysphex subfuscatus* Turner, 1917b: 323, ♀. Holotype ♀. Malawi: Mlanje (British Mus., Type Hym. 21.240).
!*Tachysphex strigatus* Turner, 1917b: 324, ♀. Lectotype ♀: East Zambia: on road Ft. Jameson to Lundazi, 4000 ft. (British Mus., Type Hym. 21.238). **Present designation; syn. n.**

The gaster is entirely red in the lectotype female of *T. strigatus,* while segments III-VI are black in the holotype of *T. subfuscatus* (pygidial area brownish red). Otherwise, both individuals are identical.

As in *T. schmiedeknechti*, the propodeum is truncate and the mesoscutum and mesopleura are strongly reticulate, but unlike this species the clypeal rim is denticulate, and the hindtibial spurs are of the usual form in *subfuscatus*.

Tachysphex erythropus (Spinola)

Lyrops erythropus Spinola, 1838: 479, "♀" = ♂. Holotype ♂ (de Beaumont, 1952a:47): Egypt (Inst. Mus. Zool. Univ. Turin).
!*Tachysphex inventus* Nurse, 1903b: 516, ♂. Lectotype ♂: India: Bombay State: Deesa (British Mus., Type Hym. 21.251). **Present designation; syn. n.**

The lectotype male of *T. inventus* displays all characters of *T. erythropus*, including the compressed forefemoral notch (without erect hair on proximal margin) and the nonagglutinate hair fringes on the gastral stera.

Tachysphex selectus Nurse

!*Tachysphex selectus* Nurse, 1909: 514, ♂. Holotype ♂: India: Bombay (British Mus., Type Hym. 21.248).
!*Tachysphex actaeon* de Beaumont, 1960: 16, ♀, ♂. Holotype ♂: Israel: Jerusalem (Mus. Zool. Lausanne). **Syn. n.**

The holotype of *T. selectus* agrees with *T. actaeon* in all characters (see Pulawski, 1971).

Tachysphex sericeus (Smith)

!*Larrada sericea* Smith, 1856: 285, ♀. Holotype ♀: Gambia (British Mus., Type Hym. 21.237). Turner, 1917d: 197 (*Tachysphex*).
!*Lyrops fluctuata* Gerstaecker, in Peters, 1857: 510, ♀. Holotype ♀: Mozambique: Tette (Zool. Mus. Berlin). Kohl, 1883b: 226 (*Tachysphex*).

Turner (1917d) and Arnold (1923a) considered these species as synonyms, but Arnold (1945) subsequently regarded them as distinct. My examination of the types has revealed that only 1 species is represented. For the characteristics of *T. sericeus* see Pulawski (1971: 416, as *T. fluctuatus*).

Holotachysphex holognathus (Morice)

Tachysphex (?) integer Morice, 1897: 308, ♀, ♂. Syntypes: Egypt: near Cairo (Oxford Univ. Mus.). *Nec Tachysphex integer* Kohl, 1892 = *Parapiagetia genicularis* (F. Morawitz), 1890.
Tachysphex (?) holognathus Morice, 1897: 434 (new name for *T. integer* Morice nec Kohl). Pulawski, 1972: 818 (*Holotachysphex*).
!*Tachysphex pollux* Nurse, 1903b: 516, ♂. Holotype ♂.: India: Bombay State: Deesa (British Mus., Type Hym. 21.516). **Syn. n.**

The holotype of *Tachysphex pollux* does not differ from Egyptian individuals of *H. holognathus*. The species was characterized by de Beaumont (1947a) and Pulawski (1972).

Prosopigastra creon (Nurse)

!*Homogambrus creon* Nurse, 1903a: 2, ♂. Lectotype ♂: India: Bombay State: Deesa (British Mus., Type Hym. 21.1,148). **Present designation.**
!*Prosopigastra (Homogambrus) acanthophora* Gussakovskij, 1933: 159, ♂. Holotype ♂: Turkmen SSR: Komarovskiy village S of Askhabad (Zool. Inst. Leningrad). **Syn. n.** Pulawski, 1965: 574 (*P. cimicivora acanthophora*).
Prosopigastra cimicivora cypriaca de Beaumont, 1954: 154, ♀, ♂. Holotype ♂: Cyprus: Zakaki (Mus. Zool. Lausanne). Syn.: Pulawski, 1965: 574.

The lectotype male of *P. creon* displays all basic features of both *P. acanthophora* and *P. cimicivora cypriaca* and is certainly conspecific. Diagnostic characters of *P. creon* are: thoracic hair erect (hair length on anterior part of mesoscutum equaling 1.5–2 midocellus

diameters), propodeal enclosure regularly ridged, knees yellow. It should be noted that in the lectotype of *P. creon* both pairs of lateral, mesosternal processes are well developed, the forewing marginal cell is short (its foremargin equaling pterostigma), and the gaster is black, except tergum I is largely brownish red.

Prosopigastra creon cimicivora (Ferton), **stat. n.**

Homogambrus cimicivorus Ferton, 1912: 406, ♀, ♂. Syntypes: Algeria: La Calle (Mus. Natl. Hist. Nat. Paris).

As shown by de Beaumont (1954), the Algerian form (*P. cimicivora*) and the Cyprian form (*P. cimicivora cypriaca* = *P. creon*) are geographic forms of 1 species. Because *P. creon* is the oldest available name, *P. cimicivora* becomes a subspecies of it.

Prosopigastra creon carinata Arnold, **stat. n.**

Prosopigastra carinata Arnold, 1922: 129. Holotype ♂: Rhodesia: Lonely Mine or Victoria Falls (Rhodesia Mus. Bulawayo).

A male from Matetsi, Rhodesiá, determined as *P. carinata* by G. Arnold, and agreeing with the original description, is conspecific with the type of *P. creon*. It displays the diagnostic characters listed above, and also the penis valve is the same as in *P. creon*. However, *P. carinata* differs from Palaearctic and Oriental populations in having dichoptic eyes (there is small space between them which equals 0.7 of a midocellus diameter). I consider this form as a subspecies of *P. creon*.

Species with holoptic eyes have sometimes been placed in the subgenus *Homogambrus*, but the holoptic-dichoptic variation in *P. creon* indicates the inappropriateness of this subgeneric scheme.

Prosopigastra nuda (Nurse), **comb. n.**

!*Tachysphex nudus* Nurse, 1903b: 515, ♀. Lectotype ♀: India: Bombay State: Deesa (British Mus., Type Hym.: 21.398). **Present designation.**

The lectotype female of *Tachysphex nudus* is actually a *Prosopigastra*. The legs and gaster are light red; the wings display a slightly infuscate transverse band; the clypeal lip is regularly arcuate, not folded; the frontal swelling is of the usual size; the mesoscutum is densely, distinctly punctate (some interspaces on the disk slightly larger than punctures); the vertex and mesoscutum have appressed hair; and the foremargin of the marginal cell is longer than the pterostigma.

Parapiagetia genicularis (F. Morawitz)

!*Tachysphex genicularis* F. Morawitz, 1890: 592, ♀. Holotype ♀: Turkmen SSR: station Pereval between Djebel and Kazandjik (Zool. Inst. Leningrad). de Beaumont, 1955: 223 (*Parapiagetia*).
!*Tachysphex (?) integer* Kohl, 1892: 216, ♂. Holotype ♂: Armenia: Arax valley (Nathist. Mus. Vienna). **Syn. n.** de Beaumont, 1947b: 677 (*Parapiagetia*).

The types of *P. genicularis* and *P. integra* are actually opposite sexes of 1 species, which is characterized by the long marginal cell (its apical truncation is much shorter than 4th abscissa of the radial sector); the erect hair on vertex and the appressed hair on mesoscutum and scutellum; the nonemarginate middle clypeal lobe in female, and arcuate middle clypeal lobe in the male. The species is related to *P. odontostoma* Kohl.

Parapiagetia substriatula (Turner), **comb. n.**

!*Tachysphex substriatulus* Turner, 1917d: 197, ♀. Holotype ♀: Pakistan: Punjab: Lahore (British Mus., Type Hym. 21.255).

Similar to *P. genicularis*, but the vertex hair is appressed, the mesoscutum is densely punctate (interspaces about 1 diameter apart), and the lateral corners of clypeal rim are sharp, prominent.

Parapiagetia erythropoda (Cameron)

!*Tachytes erythropoda* Cameron, 1889: 135, ♀. Holotype ♀: India: Uttar Pradesh: Mussooree (Oxford Univ. Mus., coll. Rothney). Cameron, 1890: pl. IX, fig. 5 (*Notogonia*); Turner, 1917d: 196 (*Parapiagetia*).
!*Tachytes denticulata* Morice, 1897: 305, ♀. Holotype ♀: Egypt: Zeitun near Cairo (Oxford Univ. Mus.). **Syn. n.** Pulawski, 1961: 86 (*Parapiagetia*).

!*Parapiagetia saharica* de Beaumont, 1956: 201, ♂. Holotype ♂: Libya: Fezzan: Brak (British Mus., Type Hym. 21.1755). **Syn. n.**

The holotype female of *Tachytes erythropoda* bears a label "*Notogonia erythropoda*" in Cameron's handwriting. This specimen is identical with *P. denticulata* (see Pulawski, 1961). Several Indian males, collected with females of *P. erythropoda*, are identical with *P. saharica*.

Larropsis (Ancistromma) obliqua (Smith), **comb. n.**

!*Larrada obliqua* Smith, 1856: 281, "♀" = ♂. Holotype ♂: Cape of Good Hope (British Mus., Type Hym. 21.331). Turner, 1917c: 291 (*Tachytes*).

Bohart and Bohart (1966) consider *Larropsis* and *Ancistromma* as distinct genera, but I cannot share their opinion. The subalar fossa is carinate below in *Larropsis* s.s. and not carinate in *Ancistromma*, but in my opinion this feature alone does not merit the generic separation of the 2 taxa, especially because all other basic structures of both are the same. *L. obliqua* belongs to species in which the subalar fossa is not carinate and is rather similar to the Palaearctic *L. punctulata* Kohl. In *L. obliqua*, however, the vertex is slightly wider than long; the clypeal lip is narrower than the middle lobe of the clypeus; and the gaster is black, with segments I and II brownish.

Tachytes vestitus (Smith)

!*Tachytes tarsatus* Smith, 1856: 297, ♀. Holotype ♀: India, ? or Pakistan (British Mus., Type Hym. 21.365). *Nec* Say, 1823.
!*Larrada vestita* Smith, 1873: 293, "♀" = ♂. Holotype ♂: India, ? or Pakistan (British Mus., Type Hym. 21.366). Syn.: Bingham, 1897: 188.
Tachytes tarsalis Dalla Torre, 1897: 685 (new name for *T. tarsatus* Smith, nec Say). *Nec Tachytes tarsalis* (Spinola), 1838.

Bingham (1897) synonymized *Tachytes tarsatus* Smith and *Larrada vestita*, and my examination of their types confirms that the two forms are the opposite sexes of 1 species. *T. vestitus* is characterized as follows: scapes, vertex, mesoscutum and tergum I with erect hair; female: vertex almost as wide as long, its hair slightly longer than midocellus diameter; mesopleura and forecorners of mesoscutum totally obscured by appressed tomentum; gastral terga I-V with silvery, apical fascia; sternum II and hindfemora without erect hair; apical lobe of hindfemora large; gastral segments I-III red, the remaining black; legs black, the inner face of foretibiae and all tarsi reddish; male: vertex wider than long, its hair equaling midocellus diameter; flagellum cylindrical; mesopleurae and forecorners of mesoscutum almost totally hidden by appressed tomentum; apical margin of sternum VIII rounded; basitarsi of midlegs as in *T. obsoletus* Rossi; gastral sterna I-II and basal half of III red, the remaining black; legs black, tarsi reddish.

Tachytes fucatus Arnold

!*Tachytes fucata* Arnold, 1951: 149, ♀. Lectotype ♀: S. Mauritania: Aleg (British Mus., Type Hym. 21.341). **Present designation.**
!*Tachytes serapis* Pulawski, 1962: 379, ♀, ♂. Holotype ♀: Egypt: Heluan (Mus. Zool. Lausanne). **Syn. n.**

The hindfemora are red in the lectotype female of *T. fucatus*, but otherwise this specimen agrees perfectly with the original diagnosis of *T. serapis* (according to which the female hindfemora are black or sometimes dark ferrugineous). As in *T. serapis*, the clypeal bevel is indistinct.

Tachytes brevipennis Cameron

!*Tachytes brevipennis* Cameron, 1900: 22, ♀. Lectotype ♀: India: Bengal: Barrackpore (British Mus., Type Hym. 21.369), present designation.
!*Tachytes maculitarsis* Cameron, 1900: 24, ♂. Syntypes: India: Bengal: Barrackpore (Oxford Univ. Mus.). **Syn. n.**

The types of *T. brevipennis* and *T. maculitarsis* are the opposite sexes of 1 species. The vertex, mesoscutum and tergum I have long, erect hair, but the hair on the scapes is short and appressed. The vertex is wider than long. The gaster and legs are black. The apical lobe of the female hindfemora is small. The male

flagellum is cylindrical, and sternum VIII is emarginate apically. The basitarsi of midlegs as in *T. obsoletus*.

Tachytes diversicornis Turner

!*Tachytes diversicornis* Turner, 1918a: 94, ♂, ♀. Lectotype ♂: Pakistan: Karachi (British Mus., Type Hym. 21.370). **Present designation.**
!*Tachytes griseola* Arnold, 1951: 149, ♀, ♂. Lectotype ♂: Ghana: Labadi (British Mus., Type Hym. 21.340b). **Present designation; syn. n.**
!*Tachytes rufitibialis* Arnold, 1951: 150, ♂. Lectotype ♂: Mali: Tillembeya (British Mus., Type Hym. 21.342). **Present designation; syn. n.**

T. diversicornis was correctly interpreted by me (Pulawski, 1962), and *T. griseolus* and *T. rufitibialis* have proven to be synonyms of this species. Unlike Asiatic, Egyptian and Sudanese specimens, the hindtibiae are reddish basally in both sexes of *T. griseolus*, and entirely red in *T. rufitibialis*.

Tachytes pulchricornis Turner

!*Tachytes pulchricornis* Turner, 1917b: 38, ♂. ♀. Lectotype ♂: Malawi: Mlanje (British Mus., Type Hym. 21.323). **Present designation.**
!*Tachytes pulchricornis* subspecies *kolaensis* Turner, 1917b: 38, ♂, ♀. Lectotype ♂: Mozambique: Kola River near E. Mt. Chiperone (British Mus., Type Hym. 21.322). **Present designation; syn. n.**

In the lectotype male of *T. pulchricornis*, the basitarsi of the midlegs are strongly modified, almost as in *T. maculicornis* Saunders (see Pulawski, 1962: fig. 119). The flagellum is cylindrical, except that the ventral face of flagellomeres VIII and IX is slightly pointed apically; the hindfemora do not have erect hair. The vertex is as wide as long. In the lectotype male of *T. kolaensis*, the flagellum is slightly more marked with yellow, and the vertex is slightly longer than wide, but these differences are of an individual, not subspecific order.

Tachytes comberi Turner

!*Tachytes comberi* Turner, 1917d: 201, ♀, ♂. Lectotype ♀: Pakistan: Karachi (British Mus., Type Hym. 21.371). **Present designation.**
!*Tachytes Patrizii* Guiglia, 1932: 475, ♂. Holotype ♂: Libya: Kufra oasis (Mus. Civ. St. Nat. Genoa). **Syn. n.**

An easily recognized species (see Pulawski, 1962).

Tachytes fulvopilosus Cameron

!*Tachytes andreniformis* Cameron, 1902b: 64. Lectotype ♀: India: Assam: Khasia Hills (British Mus., Type Hym. 21.285a). **Present designation.** *Nec Tachytes andreniformis* Cameron, 1889.
!*Tachytes fulvo-pilosa* Cameron, 1904: 297, ♀. Holotype ♀: Northern India (Oxford Univ. Mus.). **Syn. n.**
!*Tachytes fulvo-vestita* Cameron, 1904: 298 (sex not mentioned). Holotype ♂: Northern India (Oxford Univ. Mus.). **Syn. n.**

In *T. fulvopilosus* the gaster is black, without golden or silvery fasciae. The mesoscutum has golden, appressed tomentum which totally obscures the sculpture in the female and almost totally so in the male. The femora are largely and the tibiae are totally red.

Tachytes tabrobanae Cameron

!*Tachytes tabrobanae* Cameron, 1900: 23, ♀. Holotype ♀: Ceylon (British Mus., Type Hym. 21.380). **Present designation.**
!*Tachytes proxima* Nurse, 1903b: 515, ♀, ♂. Lectotype ♀: India: Bombay State: Deesa (British Mus., Type Hym. 21.376). **Present designation; syn. n.**

T. proximus is a synonym of *T. tabrobanae*. The species (only females were examined) is characterized by the dense, appressed vestiture on the mesopleura and on the dorsal side of thorax and gaster, which totally obscures the integument at the forecorners of the mesoscutum. The legs are red except for black coxae.

Tachytes modestus Smith

!*Tachytes modestus* Smith, 1856: 299, ♀. Lectotype ♀: India: Punjab (British Mus., Type Hym. 21.384). **Present designation.**
!*Tachytes maculipennis* Cameron, 1904: 299, ♂. Holotype ♂: India: Assam: Khasia (Oxford Univ. Mus.). **Syn. n.**

The lectotype female of *T. modestus* agrees with the current interpretation of this species (Tsuneki, 1964b, 1967). Diagnostic characters are: galea as long as scape; clypeal lip emarginate mesally; femora (at least apically) and tibiae red;

gastral terga I-IV with silvery, apical fascia; female gastral sternum II densely punctate throughout (including apical depression).

Tachytes modestus neglectus Turner, **stat. n.**

!*Tachytes neglecta* Turner, 1917b: 32, ♀. Lectotype ♀: Malawi: Mlanje (British Mus., Type Hym. 21.314). **Present designation.**

The type of *T. neglectus* is conspecific with *T. modestus* but this African form is sufficiently different to warrant subspecific status. It is characterized by the black femora and the dense, long hair of the underside of the hindfemora (longest hair equaling 2.5 midocellus diameters).

Tachytes borneanus Cameron

!*Tachytes borneana* Cameron, 1902a: 96, ♀. Lectotype ♀: Borneo: Kuching (British Mus., Type Hym. 21.279a). **Present designation.**
!*Tachytes banoensis* Rohwer, 1919: 8, ♀. Holotype ♀: Philippines: Luzon Is.: Los Baños (U. S. Natl. Mus. Washington). **Syn. n.**

T. borneanus is morphologically identical to *T. modestus*, but it differs from the latter in having black femora and mid and hindtibiae.

Tachytes astutus Nurse

!*Tachytes astutus* Nurse, 1909: 513, ♂. Lectotype ♂: India: Madhya Pradesh: Jubbulpore = Jabalpur (British Mus., Type Hym. 21.392). **Present designation.**
Tachytes shirozui Tsuneki, 1966: 11, ♀. Holotype ♀: Taiwan: Nantou-Hsien Pref.: Nanshanchi (Kyushu Univ. Fukuoka). **Syn. n.**

I have compared the syntypes of *T. astutus* with male specimens of *T. shirozui* received from Dr. K. Tsuneki and have found them identical in their external morphology and in the structure of their genital organs. The shape of the penis valve is characteristic for this species (Tsuneki, 1967), and male sternum VIII is not emarginate. Female sternum II is finely, densely punctate (including apical depression).

Tachytes saundersii Bingham

!*Tachytes saundersii* Bingham, 1897: 189, ♀, ♂. Lectotype ♀: Burma: Tenasserim: Haundraw Valley (British Mus., Type Hym. 21389). **Present designation.**
!*Tachytes shiva* Nurse, 1903a: 3, ♀. Lectotype ♀: India: Bombay State: Deesa (British Mus., Type Hym. 21.391). **Present designation; syn. n.**
!*Tachytes varipilosa* Cameron, 1905: 159, "♀" = ♂. Holotype ♂: Borneo: Kuching (British Mus., Type Hym. 21.340). **Syn. n.**
Tachytes formosanus Tsuneki, 1966: 12, ♂. Holotype ♂: Taiwan: Taipei-Hsien Pref.: Yangmingshan (Kyushu Univ. Fukuoka). **Syn. n.**

The female of *T. saundersii* may be easily recognized by the large punctures on sternum II (especially laterally before apical depression, where interspaces are slightly larger than punctures). The genitalia of a syntype of *T. saundersii* and of the holotype of *T. varipilosus* (which lacks the head) were examined. The penis valve of each is identical with figure 24 in Tsuneki (1966).

Tachytes saundersii suluensis Williams, **stat. n.**

Tachytes suluensis Williams, 1928: 88, ♀. Holotype ♀: Philippines: Mindanao Is.: Dapitan (Bishop Mus. Honolulu).

I examined 1 ♀ and 1 ♂ paratype of *T. suluensis* from Surinao, Mindanao. They differ from *T. saundersii* in lacking silvery, apical fascia on the gastral terga. In the male, the preapical notch of the penis valve is about twice as wide as in *T. saundersii*. I consider *T. suluensis* as a geographic form of *T. saundersii*.

Tachytes basilicus (Guérin-Méneville)

Lyrops basilicus Guérin-Méneville, 1844: 440, ♀. Holotype ♀: Senegal (Mus. Civ. St. Nat. Genoa).
!*Tachytes guichardi* Arnold, 1951: 146, ♂. Holotype ♂: Senegal: Dakar (British Mus., Type Hym. 21.1761). **Syn. n.**

I could not find any significant difference between *T. guichardi* and *T. basilicus*. For characteristics of *T. basilicus* see Pulawski (1962).

Tachytes velox Smith

!*Tachytes velox* Smith, 1856: 301, ♂. Lectotype ♂: Gambia (British Mus., Type Hym. 21.295). **Present designation.**
!*Tachytes neavei* Turner, 1917b: 13, ♂. Holotype ♂: Zaire: Lualaba River, 2500–4000 ft. (British Mus., Type Hym. 21.297). **Syn. n.**

Contrary to Turner's (1917b, in key) statement, flagellomeres II-VII are

convex below in the type of *T. velox*; therefore, the supposed difference between this species and *T. neavei* do not exist. The types of both species are identical in all details examined.

T. velox is similar to *T. basilicus* but differs from the latter by the labrum which protrudes slightly beyond the clypeal foremargin, by the black gaster and by the different basitarsi of male midlegs. The same characters distinguish *T. velox* and *T. monetarius* Smith (syntypes examined).

Tachytes argyreus (Smith), **comb. n.**

!*Larrada argyrea* Smith, 1856: 276, "♀" = ♂. Holotype ♂: Northern India, ? or Pakistan (British Mus., Type Hym. 21.250). Cameron, 1889: 143 (*Tachysphex*).

!*Tachytes melanopyga* Costa, 1893: 99, ♀. Holotype ♀: Tunisia (Ist. Zool. Univ. Naples). **Syn. n.**

!*Tachysphex debilis* Pérez, 1907: 498, ♀, ♂. Lectotype ♀ (Pulawski, 1971: 5): Persian Gulf (Mus. Natl. Hist. Nat. Paris). Syn.: Pulawski, 1971: 5.

The holotype male of *T. argyreus* agrees with *T. melanopygus* in all details of external morphology and in the structure of the genitalia (especially the penis valve). Its small size and fine sculpture indicate that *T. argyreus* is not a synonym of *T. bidens* Guss.

Tachytes pygmaeus Kohl

!*Tachytes pygmaea* Kohl, 1888: 134, ♀, ♂ (♂ = *T. argyreus* Smith). Lectotype ♀ (Pulawski, 1962: 465): Egypt (Nathist. Mus. Vienna)

!*Tachytes basalis* Cameron, 1889: 142, ♀. Holotype ♀: India: Uttar Pradesh: Mussoorie hills (Oxford Univ. Mus., coll. Rothney). Bingham, 1897: 188 (as synonym of *T. tarsatus* Smith).

!*Tachytes calvus* Turner, 1929: 556, ♀. Lectotype ♀: S.W. Africa: Okahandja (British Mus., Type Hym. 21.335). **Present designation; syn. n.**

!*Tachytes seminuda* Arnold, 1951: 153, ♀. Lectotype ♀: Mali: Tillembeya (British Mus., Type Hym. 21.336). **Present designation; syn. n.**

T. pygmaeus is easy to recognize because of its short, appressed vestiture, the glabrous propodeal enclosure, the shape of the clypeus, etc. The holotype of *T. basalis* does not differ from North African specimens described by Pulawski (1962). In the holotype of *T. calvus*, the gaster is totally red, and the hair of the pygidial area is pale brownish. In the holotype of *T. seminudus*, the vestiture does not obscure the integument at the forecorners of the mesoscutum, and the inner face of the foretibiae is red. I think that these last 2 specimens are merely extreme forms of *T. pygmaeus*.

Gastrosericus rothneyi Cameron

!*Gastrosericus Rothneyi* Cameron, 1889: 147, ♀. Syntypes: India: Bengal: Barrackpore (Oxford Univ. Mus.).

!*Gastrosericus Binghami* Cameron, 1897: 22, ♂. Holotype ♂: India: Bengal: Barrackpore (Oxford Univ. Mus.). **Syn. n.**

Contrary to Cameron's (1897) opinion, the holotype of *G. binghami* (which lacks the head) is obviously the opposite sex of *G. rothneyi*. This species is characterized by the short, appressed vestiture of the head and thorax; the long marginal cell; the middle lobe of the clypeus produced into a narrow process; the finely, distinctly punctate propodeum; the black gaster; and the yellow outer face of tibiae.

Liris aurifrons (Smith), **comb. n.**

!*Larrada aurifrons* Smith, 1859: 16, ♂. Holotype ♂: Celebes (Oxford Univ. Mus.). Kohl, 1885: 242 (*Larra*).

!*Liris nitidus* Cameron, 1913: 113, ♀. Holotype ♀: India: Uttar Pradesh: Dehra Dun (British Mus., Type Hym. 21.124). **Syn. n.**

In my opinion, *L. aurifrons* and *L. nitida* are opposite sexes of 1 species. The mandibles are entire (subgenus *Liris* s.s.); the body is black; the wings are strongly infuscate (especially in female); the gaster has weak, silvery fascia on terga I-III; the gena adjacent to eyes has golden appressed tomentum which obscures the sculpture; the propodeal sides have evanescent ridges; and sternum II is convex basally.

Liris croesus (Smith)

!*Larrada croesus* Smith, 1856: 284, ♀. Holotype ♀: Gambia (British Mus., Type Hym. 21.165). Kohl, 1894: 300 (*Notogonia*); Arnold, 1923b: 235 (*Notogonidea*).

!*Notogonia pseudoliris* Turner, 1913: 750, ♀.

Lectotype ♀: Uganda: Entebbe (British Mus., Type Hym. 21.166). **Present designation; syn. n.**
!*Motes deceptor* Turner, 1916: 253, ♀. Holotype ♀: Nigeria: Offi (British Mus., Type Hym. 21.174). Syn.: Arnold, 1923b: 235. Turner, 1917a: 320 (*Notogonia*); Arnold, 1923b: 235 (*Notogonidea*).

According to the original description the claws of *Notogonia pseudoliris* are simple, but actually the lectotype female of this species has a tooth on each claw and is otherwise identical with the type of *L. croesus*. The shape of the labrum is diagnostic in this species: it is convex, slightly protruding beyond the clypeal foremargin and has a transverse fold which is distinctly emarginate mesally.

Liris docilis (Smith)

!*Larrada docilis* Smith, 1873: 192, ♀. Lectotype ♀: Japan: Hyogo: Hokodadi (British Mus., Type Hym. 21.143). **Present designation.**
!*Larrada Tisiphone* Smith, 1873: 192, ♀. Holotype ♀: Japan: Nagasaki (British Mus., Type Hym. 21.142). *Nec Larrada tisiphone* Smith, 1857. Syn.: Tsuneki, 1964a: 221.

Tsuneki (1964a) correctly synonymized these species.

Liris jaculator (Smith)

!*Larrada jaculator* Smith, 1856: 279, ♀. Holotype ♀: India: North Bengal (British Mus., Type Hym. 21.147). Cameron, 1889: 129 (*Notogonia jaculatrix*).
!*Notogonia Chapmani* Cameron, 1900: 25, ♀. Holotype ♀: India: Himalaya (British Mus., Type Hym. 21.148). **Syn. n.**

L. jaculator is very similar to *L. memnonia* Smith (type seen) in having silvery, apical fascia on gastral terga I-IV, in having a distinct, sinuate carina on the hindtibiae, and in other characters (see de Beaumont, 1961). In *L. jaculator*, however, the subcostal vein of the forewings is reddish brown rather than black as in *L. memnonia*. The holotype female of *L. chapmani* agrees with that of *L. jaculator* in all details examined.

Liris conspicua (Smith), **comb. n.**

!*Larrada conspicua* Smith, 1856: 276, ♀. Holotype ♀: India (Oxford Univ. Mus.). Kohl, 1885: 242 (*Larra*); Bingham, 1897: 187 (*Tachytes*).
!*Notogonia pulchripennis* Cameron, 1889: 129, ♀. Holotype ♀: India: Orissa: Jeypore (Oxford Univ. Mus.). **Syn. n.** Dalla Torre, 1897: 672 (*Larra*).
Notogonia luteipennis Cameron, 1890: pl. IX fig. 2.

I consider a female bearing a label "*Notogonia luteipennis* Cam. type" in Cameron's handwriting as the holotype of *L. pulchripennis*. The former name is doubtless a lapsus for *L. pulchripennis* described a year earlier. The valid name is *L. conspicua*, whose holotype is conspecific with that of *L. pulchripennis*. The species is very distinctive: mandibles emarginate, claws unarmed, middle lobe of clypeus not emarginate; spines of foretarsal rake spatulate; propodeal sides ridged; apicoventral margin of tarsomeres V convex, but much less than in other species; gastral segments I-III and legs red; and forewings yellow, with sharply limited external band.

Lyroda argenteofacialis (Cameron), **comb. n.**

!*Astata argenteofacialis* Cameron, 1889: 151, "♀" = ♂. Syntypes: India: Bengal: Barrackpore (Oxford Univ. Mus.).

The 2 male syntypes, 1 of which bears a label "*Astata argenteofacialis* Cam." in Cameron's handwriting, actually belong in the genus *Lyroda*! The free margin of the middle lobe of the clypeus is broadly concave, but the concavity contains a mesal, arcuate lobe. The gaster is black, except that tergum I is brownish.

Bingham (1897) listed *Astata argenteofacialis* as a questionable synonym of *Lyroda formosa*. Unfortunately, I am unable to confirm this synonymy.

Lyroda formosa (Smith)

Morphota formosa Smith, 1859: 17, ♀. Holotype ♀: Celebes (British Mus.).
!*Odontolarra rufiventris* Cameron, 1900: 36, ♀. Holotype ♀: India: Assam: Khasia (Oxford Univ. Mus.). **Syn. n.** Turner, 1914: 256 (*Lyroda*).

The holotype of *Odontolarra rufiventris* is identical with *Lyroda formosa*. Both have the same clypeal shape, and gastral segments I-III are red.

Astata boops (Schrank)

Sphex Boops Schrank, 1781: 384 [♂]. Type: Austria: Pratter = Vienna (lost).

!Astata agilis Smith, 1875: 39, ♀. Holotype ♀: India: Nischiudipore (Oxford Univ. Mus.). **Syn. n.**

The holotype female of *A. agilis* agrees with the present interpretation of the common Palaearctic *A. boops*.

Astata australasiae Shuckard

!*Astata Australasiae* Shuckard, 1838: 72, ♀. Holotype ♀: New Holland = Australia, but actually Chile or Argentina (British Mus., Type Hym. 21.68b).
!*Astata chilensis* Saussure, 1854: 23, ♀. Lectotype ♀ (Parker, 1968: 848): Chile (Mus. Hist. Nat. Geneva). **Syn. n.**

A. australasiae is identical with *A. chilensis*, a South American species. The genus *Astata* is unknown in Australia and Shuckard's material was doubtless mislabeled. The type of *A. australasiae* compares favorably with the features of *A. chilensis* discussed by Parker (1968). I also examined the stigmal area of the propodeum, the basitarsi of the midlegs (foretarsi missing), and many other details.

Astata kashmirensis Nurse

!*Astata kashmirensis* Nurse, 1909: 512, ♂. Holotype ♂: Kashmir, 5000–6000 ft. (British Mus., Type Hym. 21.67).
Astata (Astata) stecki de Beaumont, 1942: 407, ♀, ♂. Holotype ♀: Switzerland: Valais: Euseigne (Nathist. Mus. Basle). **Syn. n.**

The holotype of *A. kashmirensis* is a typical representative of the common European species known as *A. stecki*. It is characterized by long flagellomeres with peculiar tyloids, the black gastral sternum II, the short mesal brush on sterna IV-VI, the concave inner face of the midcoxae, and the yellowish brown foretibiae (darkened on posterior face).

Astata compta Nurse

!*Astata compta* Nurse, 1909: 510, ♀. Holotype ♀: India: Mt. Abu, ? or Pakistan (British Mus., Type Hym. 21.58).
!*Astata absoluta* Nurse, 1909: 511, ♂. Holotype ♂: India: Mt. Abu, ? or Pakistan (British Mus., Type Hym. 21.59). **Syn. n.**

The holotypes of *A. compta* and *A. absoluta* are obviously opposite sexes of 1 species. *A. compta* is characterized by the black gaster and by the well developed, very long vestiture. In the female, the head and thorax do not have stiff, dark or silvery setae, and the basitarsi of the fore and midlegs are similar to those of *A. minor* Kohl.

Astata quettae Nurse

!*Astata quettae* Nurse, 1903a: 1, ♀, ♂ (♂ = *Astata resoluta* Nurse). Lectotype ♀ (Nurse, 1909: 510): Pakistan: Quetta (British Mus., Type Hym. 21.53).
!*Dimorpha* (olim *Astata*) *fletcheri* Turner, 1917d: 193, ♀. Lectotype ♀: India: Bihar: Pusa (British Mus., Type Hym. 21.66). **Present designation; syn. n.**
!*Astatus* (in sp.) *hirsutulus* Gussakovskij, 1927: 281, ♀. Holotype ♀: Mongolia: Lake Gashun area: Sachzhou oasis (Zool. Inst. Leningrad). **Syn. n.**
!*Astata* (s.s.) *hungarica* Pulawski, 1958: 195, ♀. Holotype ♀: Hungary: Orszent Miklós (Zool. Mus. Budapest). Syn.: Pulawski, 1965: 572.

The lectotypes of *A. quettae* and *A. fletcheri* agree with the original description of *A. hungarica*. In the lectotype of *A. fletcheri*, the frontal bristles are concentrated on the lower part of the frons, but there is some variation in the paralectotypes. The mesopleura are sparsely punctate in the syntypes of *A. fletcheri*, and the interspaces are broader than the punctures.

Astata lubricata Nurse

!*Astata lubricata* Nurse, 1903b: 514, ♀, ♂. Lectotype ♀: India: Bombay State: Deesa (British Mus., Type Hym. 21.64b). **Present designation.**
!*Astata eremita* Pulawski, 1959: 359, ♂. Holotype ♂: S.E. Egypt: Gebel Elba (coll. Pulawski). **Syn. n.**

This species belongs to the *miegi* group in which the propodeal dorsum is haired. Indian males differ from Egyptian individuals only in the following characters: hindcoxae with pale hair only, underside of hindfemora with unicolorous, sparse hair, whose length equals about 0.5 a hindfemoral diameter.

Astata (Dryudella) orientalis Smith

!*Astata orientalis* Smith, 1856: 310, ♂. Holotype ♂: India, ? or Pakistan (British Mus., Type Hym. 21.55).

Astata interstitialis Cameron, 1907: 1011, "♀" = ♂. Lectotype ♂: India: Bombay State: Deesa (British Mus., Type Hym. 21.54b). **Present designation.** Syn.: Meade Waldo, 1915: 336. Nurse, 1909: 511 (type = ♂, not ♀).

A. orientalis belongs in the *tricolor* group. The species is characterized (in the male sex) by a large, yellow frontal spot, yellow tegulae and precostal plates, and by having the basal veins of the forewings yellow; by the middle lobe of the clypeus which narrows anterad to a small, sharp point which is bent upward apically; and by the absence of a lobe on lower edge of the mandible, and no teeth or indentations on the inner edge.

According to Turner (1917d), *A. maculifrons* Cameron is also a synonym of *A. orientalis*, but I cannot agree with his opinion. Although the clypeus is pointed in both forms, the 2 are different enough to consider them as distinct species.

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BOARD OF MANAGERS MEETING NOTES

April 30, 1974

The 626th meeting was called to order at 8:08 p.m. by President Sherlin in the conference room in the Lee Building at FASEB.

Secretary.—Dr. Rupp moved that the minutes be accepted. Seconded by Dr. Robbins. Passed.

Treasurer.—The treasurer, Dr. Rupp, reported that printing expenses at Lancaster Press have been increased over 6%. He moved, seconded by Dr. Irving, that the subscription rates to nonmember subscribers be increased to $14 for U.S. and $15 for foreign. The single copy rate would be $4.50. Passed. Dr. Rupp explained our deficit budget. Dr. Robbins moved that the treasurer be authorized to liquidate securities up to $4000 in the case of absolute need. Seconded by Dr. Rupp. Passed. It was noted that we can submit a bill to DOT as soon as we have a firm price for the Symposium issue.

President-elect.—Dr. Stern requested that the committee chairmen turn in the names of their committee members, tenure, and function of the committee by the time of the annual meeting. The committees will be constituted before summer—especially Ways and Means and Policy Planning. We need to look at what the Academy is doing and its function.

New Members and Fellows.—Dr. O'Hern said that a welcome letter had been sent to new members asking them to suggest other new members.

Membership.—Mrs. Forziati presented the names of 18 people for election to fellowship. It was moved by Dr. Abraham, seconded by Dr. Robbins, that these nominations be accepted. The following fellows were elected: Paul R. Achenbach, Robert N. Goldberg, Julius Lieblein, Raymond D. Mountain, Anton Peterlin, Marjorie R. Townsend, Allan L. Forsythe, Louis S. Jaffe, Berenice G. Lamberton, Edith R. Corliss, Thomas P. Meloy, Hajime Ota, Eugene Jarosewich, Donald F. Flick, William B. Fox, Henry S. Liers, James H. Mulligan, Jr., Jenny E. Rosenthal.

Annual Meeting.—Will be held at the Cosmos Club on May 16, 1974.

New Business.—Dr. Rupp suggested that we should recognize the work that Dr. Foote does on the Journal either in the form of a letter or certificate. It was also agreed to include Mr. Detwiler's name.

It was moved by Dr. Abraham that certificates be printed that are suitable for recognition for Science Fair students, seconded by Dr. Robbins. Passed.

It was moved by Dr. Stern, seconded by Dr. Irving, that the meeting be adjourned. Passed. Meeting adjourned at 9:05 p.m.—Patricia Sarvella, *Secretary*.

NEW FELLOW

Alan S. Whelihan, Assistant Commissioner, Officer of Standards & Quality Control, General Services Adm., Federal Supply Service, in recognition of his contribution to improvement of efficiency of planning and management of the complex defense electronic systems as well as his innovative leadership in support of the Experimental Technology Incentives Program in the Federal Supply Service. *Sponsors:* Philip J. Franklin, Maurice Apstein, Roger W. Curtis.

SCIENTISTS IN THE NEWS

Contributions in this section of your Journal are earnestly solicited. They should be typed double-spaced and sent to the Editor three months preceding the issue for which they are intended.

AGENCY FOR INTERNATIONAL DEVELOPMENT

Miloslav Rechcigl, Jr., Ph.D., biochemist and research administrator, has been elected and installed as new President of the Czechoslovak Society of Arts and Sciences in America. The Society, whose foundation some 16 years ago was instigated by Einstein's distinguished disciple Prof. Vaclav Hlavaty of Indiana University, is an international cultural, non-political and non-profit organization, dedicated to the advancement of Czechoslovak studies. Active branches of the Society, which has its headquarters in the United States, may be found on virtually every continent, from the United States and Canada to Asia and Australia, from Latin America to United Kingdom and Western Europe. Membership is open to any individual, regardless of national background, interested in furthering Czechoslovak scholarship.

Apart from his purely scientific pursuits and research administrative responsibilities, for which he is generally known to his scientific colleagues, Dr. Rechcigl is recognized as an authority in the area of Czechoslovak studies. He is an expert on general and science bibliography of Czechoslovakia and East Europe as a whole, in which field he contributed several significant publications. Among others, he is the author of *Czechoslovakia and its Arts and Sciences: A Selective Bibliography in the Western European Languages* (Mouton & Co., 1964), *Czechoslovakia in Bibliography: A Bibliography in Bibliographies* (Mouton & Co., 1968), and a contributor to *East Central Europe: A Bibliographic Guide to Study and Research* (University of Chicago Press, 1969) and contributing editor to periodic "Critical Bibliography of the History of Science and its Cultural Influences", published by the History of Science Society journal *ISIS*.

As an ardent student of Czechoslovak culture he authored and edited several noteworthy books, including *The Czechoslovak Contribution to World Culture* (Mouton & Co., 1964), a two-volume set entitled *Czechoslovakia Past and Present* (Mouton & Co., 1968), and the forthcoming *Studies on Czechoslovak Culture and Society.*

He also organized the first two congresses of the Czechoslovak Society of Arts and Sciences and has been responsible for the publication program of the Society for a number of years.

At his 44 years of age, he is the youngest person elected to Presidency of the Society to date.

DEPARTMENT OF AGRICULTURE

Newsweek magazine ran an article describing the work of **Patricia Sarvella**, Geneticist, Field Crops Lab., BARC, and **Marlow W. Olsen**, formerly a physiologist with the old Poultry Res. Br., on the parthenogenesis of turkeys and Dark Cornish hens. Parthenogenetic birds are those which are hatched from unfertilized eggs.

Henry M. Cathey, Chief, Ornamentals Lab., BARC, was featured in a special section in *House Beautiful* magazine devoted to "Super Thumbs" of gardening.

Robert E. Hardenburg, Chief, Horticultural Crops Marketing Lab., BARC, has been elected a Fellow of the American Society for Horticultural Science, in recognition of his outstanding contributions and leadership in maintaining the high quality of fruits, vegetables, and flowers during marketing.

AMERICAN UNIVERSITY

Mary Aldridge has been appointed Chairman of the Honor Scroll Committee for the American Institute of Chemists.

Leo Schubert was invited to be part of an "American Chemical Society Task Force on Work-Study Programs" which was held December 6 and 7, 1974, in Washington, D.C. This meeting was organized by the American Chemical Society and the formal discussion topic was *Changes Needed in the Goals of Chemical Education.*

HARRY DIAMOND LABS

The Army's Harry Diamond Laboratories Headquarters has officially relocated to 2800 Powder Mill Road, Adelphi, Maryland, as of 10 January 1975.

The new site is located approximately five miles from the northeast boundary of the District of Columbia and represents the most modern laboratory center planned, designed and built by the U. S. Army exclusively for research and development. In January 1975, some 400 personnel engaged in mostly administrative functions joined nearly 400 scientists and engineers who relocated in February 1974. This will officially mark the transfer of the Labs from its present facilities in northwest Washington, D.C. to Adelphi. Remaining at the D.C. site and scheduled to move during the summer of 1976 are some 400 researchers engaged in such areas as fluidics, nuclear radiation effects on electronics, radar antennas, and microwave electronics.

Under the leadership of Col. David W. Einsel, Jr., Commander, and Mr. Billy M. Horton, Technical Director, the installation has become a full spectrum laboratory capable of all phases of research, development, engineering, and assistance in the establishment of an industrial production base for items evolved within the Lab.

The new site consists of 137 acres that was transferred by the Department of the Navy to the Department of the Army in late 1969. Located on the southeast corner of the Naval Surface Weapons Center, the collocation of two such well-recognized R&D activities is anticipated to reinforce each other in their similar mission assignments.

Just a few days before the U. S. Army's Harry Diamond Laboratories (HDL) moved to the new laboratory complex he helped design and promote, **Billy M. Horton** resigned as Technical Director of HDL. Thus as 1974 came to a close, so also ended the 33-year federal career of one of the Army's most distinguished science and engineering executives.

Mr. Horton left the Naval Research Laboratory and joined the Ordnance Division of the National Bureau of Standards (subsequently Diamond Ordnance Fuze Laboratories, now HDL) in 1951, and almost immediately introduced the notion of using noise-modulation in radar ranging and detecting systems. This has led to a new class of missile fuzes that are highly resistant to countermeasures. Later, he invented the basic stream interaction Fluid Amplifier that

Billy M. Horton

started the field of Fluidics, which is now being applied to a wide range of control systems, including those used for aircraft stabilization, jet thrust reversing, ordnance power supplies, safing and arming systems, industrial controls, and respirators. This invention ultimately led to HDL being designated as the Army's Lead Laboratory for Fluidics. Holder of 20 patents covering diverse fields ranging from an eye-saving Light Disrupter to a Coaxial Gravity Meter, Horton is one of the Army's most prolific inventors.

During the 13 years that Mr. Horton has been Technical Director, HDL has made many advances and accomplishments. In recognition of HDL's pioneering work in transient radiation effects, the Army's Electromagnetic Effects Laboratory was transferred to HDL and HDL was named the Army's Lead Laboratory for Nuclear Weapon Effects in 1971. HDL now has extensive nuclear simulation facilities including a nuclear reactor, several electromagnetic pulse simulators, and AURORA, a huge gamma-ray simulator funded by the Defense Nuclear Agency. Under Mr. Horton's leadership, microminiaturization and solid state technology were brought to fruition in projectile fuzes, resulting in greatly improved performance at a fraction of the unit cost. HDL also developed a unique foliage penetration radar for use in Vietnam, and several radar simulators.

During his tenure, Mr. Horton emphasized employee development by formal training, by developmental assignments, and by staff mobility. Heavy use of strong leadership by Associate Technical Directors, each having responsibilities that span the whole organization, has been a characteristic of his management.

Mr. Horton's many contributions to science and management have been recognized by the numerous awards he has received. These honors include the U. S. Army R&D Achievement Award, the U. S. Army Decoration for Exceptional Civilian Service, and the Department of Defense Distinguished Civilian Service Award. He also received the Arnold O. Beckman Award of the Instrument Society of America in 1960, the John Scott Award of the City of Philadelphia in 1966, a special award from the American Society of Mechanical Engineers in 1970 marking the 10th anniversary of the invention of fluidics, and the Inventor of the Year Award for 1971 from the Patent, Trademark and Copyright Research Institute of the George Washington University. Mr. Horton is a Fellow of the Institute of Electrical and Electronic Engineers, and a member of the Washington Academy of Science, the Philosophical Society of Washington, and the Cosmos Club.

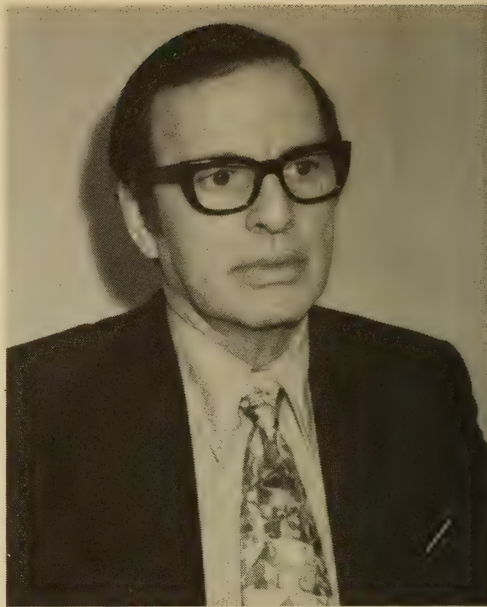
During the past decade, a major objective of Mr. Horton's and the several Commanding Officers of HDL has been the relocation of HDL to a new and permanent site. Their zeal and untiring efforts, with the aid and support of Army and Congressional leaders, has resulted in the construction of an entirely new facility at Adelphi, Md., designed and equipped specifically for R&D activities. Headquarters of HDL were

officially moved to the new site on January 10. One of Mr. Horton's regrets, aside from leaving associates and projects with which he has been closely involved, is that he will not have the opportunity to enjoy the advantages of the new site, whose design so strongly shows his influence.

Mr. Horton is retiring, he says, because he has "—a gnawing desire to become more personally involved in some technical projects that have been bugging me for a long time—some of them for several years," and he plans to devote his principal energies to those projects including a high pressure machine, foldable structures, a new kind of mechanical control system, and a rotary pump of new geometry. He says he has deep regrets about leaving "one of the best jobs in the country,—Technical Director of HDL," but feels that HDL has a "tremendous depth of talent" and that "mobility and change are good for organizations as well as for individuals."

Mr. Horton was born in Bartlett, Texas, and is a graduate of the University of Texas in 1941, BA (Physics), and University of Maryland, MS (Physics), in 1949.

He currently resides in Washington, D. C., with his wife Grace. The Hortons have two sons, Phillip Edward, 32, and Stephen Douglas, 21.



P. Anthony Guarino

P. Anthony Guarino, Associate Technical Director of the U. S. Army's Harry Diamond Laboratories (HDL), Washington, D.C., since 1958 has retired after nearly 30 years of Federal Civil Service.

Mr. Guarino joined HDL's staff as a radar specialist in 1948 and was made responsible for the design and development of specific proximity fuzes for various bombs, rockets, and mortar shells.

Over the years he was given increasingly greater responsibility for the management of technical programs which included artillery fuzing, guided projectile and missile fuzing and more recently special purpose radars. In 1969 he received the Army's R&D Achievement Award for his efforts during one such technical program.

An author of nearly fifty technical reports on fuzing, radar, radio, and countermeasures, Mr. Guarino has served as consultant on numerous working groups including the NATO team on Mutual Weapons Development and the United States-United Kingdom-Canada-Australia Technical Cooperation Program. In the 1960's he represented the U. S. Army in the National Space Program Research and Development Briefings, and in recent years has been an Army Consultant to the Re-Entry Systems Advisory Group of the U. S. Air Force Space and Missile Systems Organization.

Born in Cambridge, Mass. in 1912, Mr. Guarino received his B. S. in 1935 from Massachusetts Institute of Technology, and in 1940 a M.S. (Physics) from University of Notre Dame. He currently resides in Rockville, Md. with his wife Betty.

Ernest M. Levin

Ernest M. Levin, a chemist at the National Bureau of Standards who gained an international reputation for his work on glass and ceramics, died of cancer on August 7, 1974, at the Veterans Administration Hospital in Washington. He was 59.

Mr. Levin's specialty was high-temperature phase diagrams. These are analyses of chemical reactions of materials subjected to intense heat. The diagrams of these reactions enable research and industrial scientists to predict how materials will behave under certain conditions.

In recent years, Mr. Levin had been chief author of "Phase Diagrams for Ceramists." The book is known as "the bible" for virtually all scientists working with glass and ceramics.

The diagrams have industrial applications ranging from the construction of kilns in steel mills to the most delicate optical systems. They have also been used in developing new types of glasses, magnetic devices for computers, devices for converting electrical energy into mechanical energy, as in phonograph recordings and tape recorders, and in the manufacture of single crystals for laser beams and optical communications systems. They have also been used in developing materials with high resistance to corrosion in the petrochemical and steel industries.

Mr. Levin's latest project, completed shortly before his death, was the compilation of nearly 1,000 new phase diagrams for a new edition of "Phase Diagrams for Ceramists."

For this and his earlier work, Mr. Levin was nominated for the 1974 Department of Commerce Gold Medal Award, the highest honor the department can confer. The medal was presented to his family at a ceremony this fall.

"This is the capstone of a career which began with research and concluded with a combination of compilation (of phase diagrams), evaluation and research," says Dr. John B. Wachtman, chief of the inorganic materials division of the Bureau of Standards.

"The point is that evaluation is not something just a clerk can do. It's an act of analysis and judgment which only an experienced and highly qualified man can do."

Besides the Gold Medal, Mr. Levin's awards included the Bronze and Silver Medals of the Commerce Department and the Special Achievement Award of the Bureau of Standards. He was the seventh person to receive the Presidential Citation from the American Ceramic Society in the 75-year-history of that organization and he was also a recipient of the society's S. B. Meyer Award.

Mr. Levin was a member of numerous profession organizations, including the American Ceramic Society, the American Institute of Chemists, the British Ceramic Society and the American Chemical Society.

Mr. Levin was born in Detroit and graduated from the University of California at Los Angeles in 1935. He earned a master's degree from UCLA and began his 37-year-career with the National Bureau of Standards at Riverside, Calif., in the same year. He was transferred to Washington in 1944. He was a GS-15 with a salary of about $32,000 a year at the time of his death.

In private life, Mr. Levin enjoyed gardening and practicing and teaching yoga.

Survivors include his wife, Doris, of the home at 7716 Sebago Rd., Bethesda; two sons, Fred and Robert, both of the home; a daughter, Ellen Share, of Brookville; his parents, Dr. and Mrs. N. P. Levin of Los Angeles, and a grandchild.

Paul E. Howe

Paul E. Howe, 89, a retired Army colonel and well known nutrition authority, died on Sept. 27, 1974 in Washington after a short illness.

Col. Howe was a native of Chicago and studied at the University of Illinois, earning bachelor's, master's and doctoral degrees. He taught at Columbia University and at the Rockefeller Institute at Princeton, N. J., before coming to Washington in 1924 to take a job with the Department of Agriculture as a research nutritionist.

During World War I he had been one of the Army's first nutrition officers. In World War II he was head of the division of foods and nutrition of the surgeon general's office. During the postwar occupations of Germany and Japan, he worked on the nutritional needs of the civilian populations of those two countries.

Col. Howe also served as a nutritional adviser to the Bureau of Prisons and developed a way of evaluating and maintaining nutritional adequacy in institutional feeding.

Col. Howe retired from the Department of Agriculture in 1955, but continued to work as a consultant for, among others, the California Department of Corrections and as a Fulbright lecturer at the Instituto Nazionale Della Nutrizione in Rome.

He is survived by his wife of 61 years, Harrient Rinaker of Washington; two daughters, Clarissa Beerbower of Westfield, N. J., and Elizabeth Hyde of Bly, Ore.; five grandchildren and five great-grandchildren.

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CONTENTS

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National Geographic Society	Alexander Wetmore
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Mathematical Association of America	Inactive
D.C. Institute of Chemists	Miloslav Recheigl, Jr.
D.C. Psychological Association	Delegate not appointed

Delegates continue in office until new selections are made by the respective societies.

Technology Assessment and Public Wisdom

Joseph F. Coates

Office of Technology Assessment, U. S. Congress

On the average of once a month since World War II, the United States public has been exposed to an incident of technological failure, alarm, concern or major uncertainty sufficiently important to merit attention in the national press. Virtually every sector of our economy has contributed to this relentless flow of public concerns. Pesticide residues in Christmas cranberries, nerve gas stored on the flight path of the Denver Airport, mercury residue in tunafish, regional electric power blackouts, chronic water pollution from petroleum with occasional major spills, faulty vehicles and faulty road design, toys that are unsafe, baby clothes that are highly combustible, convenience packages with inconvenient aesthetic side effects, and it goes on, and on, and on.

Reaching into recent files of such incidents, I find, for example:

1. *New York Times,* December 25, 1974, an alleged "murder for insurance" scheme in Lexington County, South Carolina, closely modeled on a recent television program.
2. *New York Times*, December 24, 1974. Four pound cans of pimentos were recalled by the Food and Drug Administration because of a possible public health risk since they might have been processed improperly and contain potentially dangerous micro-organisms.
3. January 11, 1975. A major manufacturer of baby food recalls a quarter of a million cases of dry food. It may contain metal fragments due to the failure of process machinery.
4. January 17, 1975. New York State Supreme Court jury finds New York City's liability up to 65% on claims totaling more than $50 million for a gas explosion in a building, fatal to a dozen people.
5. *New York Times*, January 2, 1975. Many University students are reported using U. S. food stamps as a form of scholarship, an innovation in subsidies not intended by the legislation.

The incidents may be large or small. They may be of national or local scope. They may hit anyone, anywhere, randomly or systematically. What is the underlying situation that makes such incidents not only more frequent but of growing public concern? Basically I believe it is because we moved past a situation in which man was in constant struggle with a dominant natural environment. Until very recently nature could not only recover from man's intrusions but could thwart the goals and intentions of most human endeavors. Within the last two generations, that situation has nearly reversed in the United States.

The material in this paper is the responsibility of Mr. Coates and does not represent the position of any government agency or the U. S. Congress.

The life of most Americans is within a fully man-made world. Most of the readers of this article are surrounded entirely by human artifacts and nothing else. We depend on them for food, shelter, clothing, work, entertainment and leisure. Only on those special occasions when we purposely set aside the time, and go to the trouble, do we encounter something like nature in the raw. Even then, it is likely to be a simulated or man-sustained situation.

While our world has changed, the rules and regulations reflected in our institutional, personal and organizational orientation toward the world have become obsolescent. They reflect categories and approaches appropriate to a parochial gross struggle against a universally powerful nature. The enterprises of man have reached a stage where their scale, their scope, the size of investments; the speed with which technological change permeates society; the relative irrevocability of big enterprises—all demand conceptually fresh approaches in the social management of technology. Crucial to that new approach are foresight, feedback and flexibility. It is literally true that federal highway programs, be they good or bad, are set in concrete, and that concrete, except in rare occasions, will remain set for 30 to 60 years. The building of a major power plant, whether conventional or nuclear, is an event which is likely to be functionally irreversible for a long time. The opening of a new waterway, the immigration of a new pest or predator, the construction of a new high rise building, all make more or less irrevocable commitments to the future. Even in the social area, the institution of a new social program, new benefits, new legislation, or new regulations tend to lack flexibility and responsiveness which permits timely compensation for error, mistake or shortfall.

How did this come about? In my view, the technological economic planning in the United States has been overwhelmingly premised on an affirmative response to very little more than three questions:

- Is the technological objective feasible?
- Will it sell? That may be via direct competition in a free market, or the competition may be for public funds and government programs.
- Is it safe?

Reflection on any of the above illustrative shortfalls and failures of technology does not suggest that these criteria are not good, but rather that they are inadequate. Many of the most significant consequences in our highly technologized society do not happen immediately. They may be slow in building (such as the evil thoughts planted by a television program), they may be convergent (as when new contraceptive technology, increased levels of education, and prosperity promote the women's liberation movement), they may be incidental (as when the one in a few thousands of a broadly dispersed technological item fails) or they may be catastrophic when an otherwise well functioning system collapses as a result of an administrative or technological glitch (the crash of a giant passenger airplane).

The above criteria must be extended and expanded so that the range of considerations which enter into public and private decision making are appropriate to our world. Before any organizational, institutional, economic or technical remedies or controls should be instituted, we must better understand the future implications of any particular technological development and the policies for its management. Technology Assessment is one approach to providing this expanded foresight. It may be defined as a class of policy studies examining the fullest range of impacts of the introduction of a new technology or the expansion of a present technology in new or different ways. It is an analysis of the total impact of a technology on society. Technology assessment, therefore, is much broader than the traditional technological planning, which is usually based on meeting some sort of

potential or felt need, of satisfying or creating some new market.

Technology assessment does not deal only with the dark, the negative, the unfortunate consequences of technology. It is a tool for optimizing the benefits and penalties that technology may bring. Why is it, for example, that the widely touted benefits implicit in new approaches in education through computers and telecommunications have been such a disappointment? Why is it that cable television in the U. S. remains underdeveloped and relatively uninteresting? Why is it that the modern technological advances applicable to architecture, design and building repeatedly end up in junky, malfunctioning, aesthetic insults? The answer, in part, has to do with the inadequacy of the three traditional techno-economic planning criteria. With every actor in the drama optimising in his own self interest, with nobody in charge, with no one having a synoptic grasp of the matter, the yield must be less than the best.

A third class of activities that will also benefit from holistic evaluation are those situations in which there is a belief that physical, biological, or social technology offers some opportunity to alleviate or deal with a public issue. Problems of welfare, education, delinquency, crime, resource control, energy, conservation, and others in the limitless stream of issues are potential candidates for illumination by technology assessment.

Three major developments ought to be considered with regard to technology assessment, since the term was first coined and used by the Subcommittee on Science, Research and Development of the House of Representatives' Committee on Science and Astronautics.

First, there is the development of a comprehensive systematic modestly funded program of technology assessment at the National Science Foundation (see Table 1). Second is the attempt at technology assessment by a number of Federal agencies, most of which so far involve single attempts with little follow-through. There are of course the studies which qualify as more or less comprehensive or partial technology assessments, undertaken under other names by federal agencies for many years. These have been reviewed and analyzed through 1971 (reference I). And finally, there is the formation of the Office of Technology Assessment in November, 1973, as a new agency of government specifically dedicated to assisting Congress in meeting its study needs. Those needs relate to four Congressional functions; legislation, oversight, budget, and policy formulation.

How To Do a Technology Assessment

It would be convenient were there a formula or prescription for technology assessment. Unfortunately it is unlikely that a general formula will ever be available, since the approach to and content of an assessment is determined by three primary considerations: the subject; the budget; and the primary user. Clearly, different factors are important in the technologies, consequences, and policies of genetic engineering, weather modification, and airport siting. Consequently different conceptual tools are certain to be appropriate in a holistic analysis of each of them. With regard to budget, a technology assessment may be done at a wide range of funding levels. One analyst or a panel of wise men working for a few months could do one kind of job for \$20,000, while a major think tank study team with \$500,000 and two years would do a quite different job. Different techniques would be appropriate for these different study efforts which might nevertheless be dealing with the same subject matter. The assessment of earthquake prediction technology, noted in Table I, is being paralleled by an effort one-tenth as large by a single investigator. The third major determinant influencing the scope of the study is its principal user. The range of impacts and consequences of a drug considered by a drug company,

Table 1—NSF Completed and Current Technology Assessments.

Agency	Title	Starting Date/ Duration	$ Amount
Completed Projects			
Stanford Research Institute Menlo Park, Calif.	Technology Assessment Study of Winter Orographic Snowpack Augmentation in the Upper Colorado River Basin	01/12/71 14 months	179,479
University of Oklahoma Norman, Okla.	A Technology Assessment of Offshore Oil Operations	07/01/71 28 months	288,600
Hittman Associates, Inc. Columbia, Md.	Evaluation of the Ecological, Resources and Socio-Economic Impacts of Advanced Automotive Propulsion Systems	07/01/71 34 months	326,129
Columbia University New York, N.Y.	The Automobile and the Regulation of its Impact on the Environment	09/01/71 24 months	310,000
Kansas State University Manhattan, Kan.	Political and Scientific Effectiveness in Nuclear Materials Control	06/01/70 24 months	254,000
Virginia Polytechnic Institute Blacksburg, Va.	Assessing the Implementation Aspects of Technology for the Disposal of Solid Waste	06/11/71 10 months	40,000
University of Michigan Ann Arbor, Mich.	Assessing the Impact of Remote Sensing of the Environment	06/01/72 15 months	141,500
Rensselaer Polytechnic Institute Troy, N.Y.	Technology Assessment for Cable Television	11/15/72 12 months	48,900
Current Projects			
National Academy of Sciences Washington, D.C.	Assessment of Biomedical Technology	06/15/71 12 months	86,800
University of California Los Angeles, Calif.	A General Approach to Risk-Benefit Evaluation for Large Technological Systems	06/01/73 18 months	343,600
The Futures Group Glastonbury, Conn.	Technology Assessment of Geothermal Energy Resource Development	07/16/73 12 months	191,882
Arthur D. Little, Inc. Cambridge, Mass.	The Cashless-Checkless Society: An In-Depth Technology Assessment	09/28/73 18 months	220,706
Midwest Research Institute Kansas City, Mo.	A Technology Assessment of Biological Substitutes for Chemical Pesticides	01/02/74 12 months	113,700
Midwest Research Institute Kansas City, Mo.	An In-Depth Technology Assessment of Integrated Hog Farming	01/02/74 18 months	212,879
University of Minnesota Minneapolis, Minn.	Technology Assessment of Conversion from the English to Metric System in the United States	10/01/73 18 months	179,100
Haldi Associates, Inc. New York, N.Y.	Technology Assessment of Alternative Work Schedules	11/01/73 18 months	207,400
Braddock, Dunn and McDonald, Inc. Vienna, Va.	Technology Assessment of Alternative Strategies and Methods for Conserving Energy	11/15/73 18 months	238,638
Stanford Research Inst. Menlo Park, Calif.	A Technology Assessment of a Hydrogen Energy Economy	07/01/73 12 months	122,200
Arthur D. Little, Inc. Cambridge, Mass.	Technology Assessment of Terrestrial Solar Energy Resource Development	07/16/73 12 months	246,664
Stanford Research Inst. Menlo Park, Calif.	A Technology Assessment of Earthquake Prediction	06/01/74 12 months	283,500

a state agency, the FDA, the White House, and the Congress are increasingly wide in scope because of the increasing range of responsibility of each of those groups. Since experience and general principles preclude any common set of tools or techniques applicable to the examination of the impacts of all technologies, it is important to note that there are common features to all technology assessments. The organization of an effective work plan must take these

common modules or elements into consideration if the goals of a technology assessment are to be met and it is to be more than a cost-benefit, marketing, feasibility or systems study.

Ten Modules of a Technology Assessment

(1) *Definition of the problem, the technology, issue or project to be assessed.* The client or user of a technology assessment is likely to be unclear as to what the problem is. Hence close examination and reworking is in order to put it in a proper form to permit a useful study with a decision-related output.

(2) *Definition of alternative systems to be examined.*

(3) *The unfolding of impacts.* The identification of impacts requires a combination of experience, skills, imagination, and creativity. There literally are no complete models, paradigms, or algorithms by which one can identify the consequences of a given technology. In some cases the technology itself may suggest where to look for impacts. For example, with geothermal energy, the physical system offers a logical path along which to look for effects. In some cases the technology may be so diffuse, as with the four-day workweek, that one must go to one or another "methods of exhaustion" to identify impacts.

(3) *Evaluation of the significance of impacts.* Many qualitative and quantitative tools may be brought to bear here, including tools of economic analysis, social surveys, scaling techniques, and others, but one can expect that the evaluation is likely to be a mixture of relatively hard and soft outputs. One must be on guard that the study team not limit its evaluation to what is easy to do, at the price of ignoring the crucial and difficult.

(5) *The decision apparatus* relevant to the problem should be identified explicitly and the range of responsibilities of individual components defined as far as is feasible.

(6) *Defining options and alternatives open to the decision apparatus* is something of a creative enterprise. One must attempt to innovate with regard to action options and alternatives and to relate to the apparatus at hand. The failure to do this often leads to vague, uncertain or useless options and conclusions.

(7) *Parties at interest with regard to a particular technology.* It is important to identify who in fact or in principle has a stake in the technology and in its possible impacts and consequences. This is important from an analytical view in helping to identify impacts and consequences. It is also important from a decision point of view by indicating who may influence the range and kind of action options which the decision maker has before him. The parties at interest after all are those who will or should have the strongest influence on the decision apparatus.

(8) *It is important to recognize and analyze the impacts of variations on the technology under consideration.* However, there is another set of technological alternatives which must be considered and these are what one might call *macro alternatives*. For example, the various ways of removing oil from the north slope of Alaska would not comprise macro alternatives but rather systems alternatives. A macro alternative might be the development of geothermal resources, or the cutting down on the demand for energy.

(9) *Exogenous factors should have a prominant place in any technology assessment.* By exogenous factors I mean those changes in society, its goals, its orientation, or its technology which could have an influence on the primary technology or factors interacting with it. These exogenous factors may vary anywhere from another new technology itself, to an economic upturn or downturn, change in the international situation, or modification of legislation. Again, as with impacts, the identification of exogenous variables is a partially analytical and partially creative exercise. The shift in Arab oil policy is an example of the failure to anticipate

a potential exogenous factor relating to energy policy and plans.

(10) *One must examine all the above to come to some set of conclusions,* possibly to some recommendations. In general, it is best not to come to a precise and definitive single set of recommendations. A set of alternatives and an analysis of the consequences is most useful for the decision maker.

In general, a technology assessment cannot be conducted as a once-through exercise in filling out each of the categories mentioned above. Experience suggests that any particular assessment study should be done three times over. The first time to define and understand the problem, the second time to do it right, and the third to burnish the results, fill in the detail, and to bring the report to the best possible state within the available time, budget, and manpower. This recycling is important to keep in mind since many uninitiated schedule their work to do the study once and make no allowances for response, review, criticism, or their own learning process.

The Consequences of Technology Assessment

The consequences of technology assessment are important to consider because the actions open to the decision maker may have a profound influence on the scope and depth of the examination and the very organization of a comprehensive technology assessment. Among the outcomes of a successful technology assessment may be the following:

(1) Redefining the issue or restructuring the problem.

(2) Modifying the project or technology to reduce disbenefits or to increase benefits.

(3) Defining a monitoring or surveillance program with regard to the technology as it becomes operational.

(4) Stimulating research and development, to define risks more reliably, forestall anticipated negative effects, identify alternative methods of achieving the goal of the technology, and identify feasible corrective measures for negative effects.

(5) Identifying regulatory, legislative, or other control needs.

(6) Identifying needed institutional changes or innovations.

(7) Providing sound inputs to all parties at interest.

(8) Preventing a technology from developing (an unusual but not impossible outcome).

(9) Defining a set of intervention experiments or stepwise implementation of the technology.

Technology Assessment in Government

Table 1 outlines the principal completed or ongoing technology assessment projects sponsored by the National Science Foundation. As the federal agency with lead responsibility in the field the principal goals of its program are:

(1) To sponsor high quality substantive assessments relevant to policy in order to demonstrate the value and practicality of the concept and to effect public policy in a useful way.

(2) To promote the development of methodology and techniques for assessment.

(3) To develop individual and institutional competence to undertake assessments for other agencies.

(4) To support state-of-the-art review activities and to assist in organizing and consolidating this new field.

Needless to say, the full consequence of even the best technology assessment may be slow in developing, inasmuch as it is one input into the continuing policy process.

Other federal agencies, alert to the significance of technology assessment, have sponsored projects of direct interest to their missions. For example, NASA is currently sponsoring a technology assessment of inter-city transportation and another one on fuels alternative to petroleum.

Several years ago, jointly with the Department of Transportation (DOT), it funded with Civil Aviation R&D Study which assessed a number of civil air systems. The Postal Service has sponsored a combination technological forecast and technology assessment. The Office of Coal Research has also been in the field.

DOT is about to receive the final report on a project stemming from the Congressional rejection of support for the SST. That study looks at the climatic implications of atmospheric pollution. NIH has made some rudimentary movement into TA with studies of cardiac transplant and the artificial heart.

Municipal government has been indifferent to the concept, but several state agencies have become alert to TA. The State of Hawaii has sponsored an assessment of harvesting manganese nodules from the ocean. The Western Interstate Nuclear Board has done an assessment of Project Plowshare. The Port of New York Authority sponsored an assessment by the National Academy of Sciences of a proposed extension of the Kennedy Jetport into Jamaica Bay. The West Virginia legislature sponsored an assessment of strip mining conducted by the Stanford Research Institute.

Of the international agencies, only the OECD is significantly involved in technology assessment, although the UN and the EEC (European Economic Community) are taking the preliminary steps in this direction. Among foreign governments the Japanese are most conspicuously active. The Swedish, Canadian, British, and German governments directly or through associated institutions are active in technology assessment (references 2,3).

The Office of Technology Assessment

Perhaps the most significant development in the United States in this field is the formation of the Office of Technology Assessment (OTA) established by the Technology Assessment Act of 1972 (Public Law 92–484). OTA's mission is to examine the many ways, expected and unexpected, in which technology affects people's lives. OTA consists of a non-partisan Congressional board, comprised of six Senators and

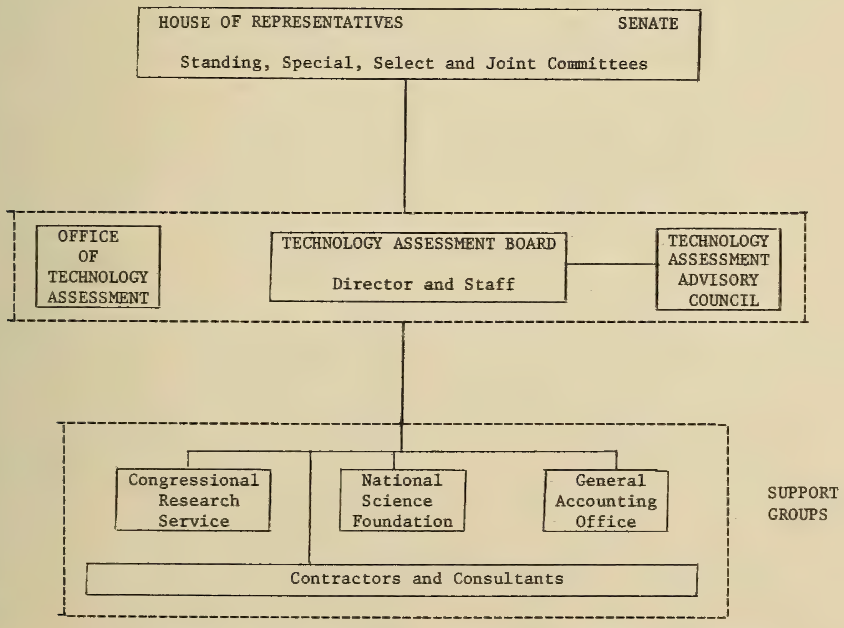

Fig. 1. Organization relationships of the Office of Technology Assessment.

six House Members, which sets policy; a Director, who also is a member of the board, a Deputy Director and other officers and employees, and a twelve member citizens advisory council, which includes as ex-officio members the Comptroller General of the United States and the Director of the Congressional Research Service of the Library of Congress (see Table 2).

The chairmanship of OTA's Congressional board rotates between the Senate and the House in alternate Congresses. The first Board Chairman was Senator Edward M. Kennedy, Democrat of Massachusetts. The first Vice Chairman was Congressman Charles A. Mosher, Republican of Ohio. With the opening of the 94th Congress in 1975 the current Board Chairman is Congressman Olin Teague, Democrat of Texas, and the Vice Chairman is Senator Edward Case, Republican of New Jersey.

The Director of OTA is Emilio Q. Daddario, a former Member of Congress who was instrumental in the development of the Technology Assessment Act. The Deputy Director is Daniel V. DeSimone, a former White House science policy assistant. The Chairman of the citizens advisory council is Dr. Harold Brown, President of the California Institute of Technology. The Vice Chairman is Dr. Edward Wenk, Jr., of the University of Washington.

Early in 1974, OTA began its work for Congress by launching assessments in six areas: food, energy, the oceans, materials resources, health, and urban mass transportation (reference 4).

Implications of Technology Assessment For Business

As part of the continuous tightly knit fabric of American society business will be affected by technology assessment to

Table 2.—Office of Technology Assessment.

Emilio Q. Daddario, *Director*
Daniel De Simone, *Deputy Director*

TECHNOLOGY ASSESSMENT BOARD

Olin E. Teague, Texas, *Chairman*
Clifford P. Case, N.J., *Vice Chairman*

Edward M. Kennedy, Mass.	Morris K. Udall, Ariz.
Ernest F. Hollings, S.C.	George E. Brown, Jr., Calif.
Hubert H. Humphrey, Minn.	Charles A. Mosher, Ohio
Richard S. Schweiker, Pa.	Marvin L. Esch, Mich.
Ted Stevens, Alaska	Marjorie S. Holt, Md.

Emilio Q. Daddario

ADVISORY COUNCIL

Dr. Harold Brown, *Chairman,* President, California Institute of Technology.
Dr. Edward Wenk, Jr., *Vice Chairman,* Director, Program in the Social Management of Technology, University of Washington.
Mr. J. Fred Bucy, Executive Vice President, Texas Instruments, Inc.
Mrs. Hazel Henderson, author and lecturer on environmental and social issues, Princeton, New Jersey.
Mr. Lester S. Jayson, Director, Congressional Research Service, Library of Congress.
Mr. J. M. (Levi) Leathers, Executive Vice President, DOW Chemical Corporation.
Dr. John McAlister, Jr., Associate Professor, Department of Engineering-Economic Systems, Stanford University.
Dr. Eugene P. Odum, Director, Institute of Ecology, University of Georgia.
Dr. Frederick C. Robbins, Dean, Case Western Reserve University School of Medicine (Nobel Laureate).
Mr. Elmer B. Staats, Comptroller General of the United States.
Dr. Gilbert F. White, Director, Institute of Behavioral Science, University of Colorado.
Dr. Jerome B. Wiesner, President, Massachusetts Institute of Technology.

its very core to the extent that government applies any new techniques of foresight, feedback, flexible control, policy analysis, and long range planning. As a minimum, technology assessment will become another, and in my judgement, crucial long range planning tool in business. The concept is central to the anticipation of new markets, future institutional and regulatory environments, and such elements of business as work force, resources, and public attitudes. Inasmuch as a large enterprise must have a long time horizon (automotives, steel, resources, telecommunications, aircraft, chemicals, housing, transportation, marketing chains) they will need technology assessments of their own, for the very reason that government is using this tool, to set policy, plans, and programs.

By and large, industry to date has been indifferent, hostile, or confused about technology assessment. One study done by a management consulting firm revealed that large numbers of corporations claimed they were doing T.A. but on closer examination virtually none were. They were confusing it with feasibility studies, market research, technological forecasting, product evaluation, and a variety of other well established business tools.

On the other hand, many who have understood the goals and objectives of T.A. have highlighted a potential risk in excessive scrutiny and hyper concern for potential risks in leading to a general ambience of technology arrestment. In general, I think one can anticipate that if any major bureaucracy with vast commitments of funds, labor and investment in on-going enterprises either willingly or unwillingly has its preconceptions and tacit assumptions laid bare and examined, trouble must result. One sees this to some extent in the environmental impact statement process, which attempts to get to the core of the environmental implications (a partial technology assessment) of many activities. I believe this turbulance is a necessary part of a transition process in which a systematic look to the future implications of technology will become integrated into earlier and earlier stages of public planning. As Ian Wilson of the General Electric Company has pointed out, any major corporation or large enterprise may exhibit a variety of responses to external and internal pressures for institutional change. These may vary from last ditch resistance and most begrudging acquiescence on the one hand to early recognition and integration of the inevitable consequences of change into the corporate operation with a view to taking positive advantage of the inevitabilities. An enthusiastic view of the role of technology assessment has been presented by Carl Madden, Chief Economist of the Chamber of Commerce of the United States, in a recent National Planning Association monograph (reference 5).

Conclusion

Technology assessment, whether or not it flourishes under that rubric and in its present institutional context, is less important than the fact that it is a concept and an activity inevitably to become a part of the public and private planning process. It is associated with a number of long-term trends in American government and public and private bureaucracies. Among these are: (a) an awareness of alternative futures and long range planning; (b) the use of planning and policy studies and study groups; (c) within government, the institutionalization of foresight, as in the requirements for environmental impact statements; (d) the redress of a long term imbalance in analytical and support capabilities between the Executive and the Legislative branches.

Additional Readings

Technology Assessment, A Quarterly Journal of the International Society for Technology Assessment (ISTA), which contains general and in-depth articles on the methodology, organization and activities involving technology assessment. The Society's Washington address is P.O. Box 4926, Cleveland Park Station, Washington, D. C. 20008.

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Energy Under the Oceans, A Technology Assessment of Outer Continental Shelf Oil and Gas Operations, Don E. Kash *et al.*, University of Oklahoma Press, Norman, Oklahoma, 1973.

The Impacts of Snow Enhancement: Technology Assessment of Winter Orographic Snowpack Augmentation in the Upper Colorado River Basin, Leo W. Weisbecker, Stanford Research Institute, University of Oklahoma Press, Norman, Oklahoma, 1974.

The Automobile and the Regulation of its Impact on the Environment, Legislative Drafting Research Fund, Columbia University, New York, New York (forthcoming, University of Oklahoma Press).

A Technology Assessment of Geothermal Energy, The Futures Group, Glastonbury, Connecticut (forthcoming).

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"Technology Report/OTA Works to Produce Track Record with Six Major Projects," John F. Burby, *National Journal Reports*, Washington, D. C., September 28, 1974, Vol. 6, No. 39, 1454–1464.
5. *Clash of Culture: Management in an Age of Changing Values*, Carl H. Madden, National Planning Association, Washington, D. C., October 1972, Report No. 133.

Washington Junior Academy of Sciences— Christmas Convention

An annual highlight of the Washington Junior Academy of Sciences program is the Christmas Convention featuring the student presentation of research papers selected from written theses submitted by students of the public, private, and parochial high schools in the Greater Washington Metropolitan Area. The 1974 Convention was a two-day activity held at Georgetown University on December 13th and at the National Zoological Park on December 14th. The program was organized and presided over by David Leighton, Vice-President, WJAS, and a senior at Washington-Lee High School.

Both opening day sessions were devoted to the presentation of the ten papers. The papers were judged by a panel of five scientists and engineers recruited by Dr. Russell W. Mebs, a Senior Advisor to the WJAS. In addition to Dr. Mebs (physicist), the panel consisted of Miss Lucille V. Peoples (mathematician), Mrs. Mary Izzard (botanist), Mr. Joseph Krostein (chemical engineer) and Mr. Karl Izzard (highway engineer).

The morning session of the second day was spent on a highly informative behind-the-scenes tour of the Zoo, led by Mr. Mike Morgan of the Zoo staff. In the afternoon an awards ceremony was held at which Certificates of Commendation were presented to the ten

student speakers and to Mr. Morgan. Cash prizes of $5.00 to $20.00 were then presented to the authors of the four papers ranked highest by the judges. First prize was awarded to Reginald Jenkins, second to Ursula Schwebs, third to John Foltz, and fourth to Jandel Allen.

The program closed with a vote of appreciation to the judges who had devoted the whole of Friday to their deliberations and to both the NUS Company and the TRW Company who had donated the money which made possible the granting of financial awards.

Abstracts of most of these papers are presented below.

—**Mrs. Elaine G. Shafrin,** *Chairman*
WAS Committee on Encouragement of Science Talent

THE EFFECT OF CHALONES ON TRANSFORMED LYMPHOCYTES

Reginald Jenkins

Gonzaga High School, Washington, D.C.

Chalones are specific mitotic inhibitors responsible for the controlled division of normal lymphocytes. Bullough and Laurence proposed that epithelial cells make a specific inhibitor of the mitosis of the cell. Subsequent studies by others have indicated that these mitotic inhibitors might also be obtained from melanocytes, granulocytes, kidney cells, cells of the lense, and from spleen and thalmus glands.

Studies of the physiological effects of chalones on lymphocytes stimulated by phytohaemagglutinin (PHA) have shown that the chalone is most effective when introduced during stimulation of the lymphocytes with PHA (day zero). This brings to light the question of how chalones effect the morphology of stimulated (transformed) lymphocytes.

This experiment examines various morphological changes of transformed lymphocytes in respect to cell size and vacuolization when treated with spleen chalone.

ELECTRICAL CHARACTERISTICS OF PLANTS

Ursula Schwebs

Washington-Lee High School, Arlington, Va.

This science project started last year with an attempt to verify claims made by Cleve Backster and others that "plants have emotions similar to those of human beings." I found that the "Backster effect" does not exist, plants do not have feelings as we know them, but plants generate an electric field that interacts with its environment. Results were reported in HARPER's, June 1973.

The 1973/1974 experiments were designed to gain a better understanding of the selfgenerated electrical potential of plants of different origin, structure and leaf texture. Feeding was varied from distilled water to salt solutions and combination of minerals. Reactions to different feedings and to changes in the external environment were measured, such as humidity, temperature, light, sound, and electrostatic fields.

My experiments show evidence that all living cells generate an electrical potential. Through internal conductance paths, measured as resistance, a small current flows through the plant. Absolute

values vary with the plant structure, its water content and external humidity and electrical fields. The internal current is related to growth. Light influences the internal potential and current the same way as externally applied voltage. Both can increase, decrease or reverse the internal potential. Both start an electro-chemical reaction that continues for about 20 minutes after light or voltage is turned off. Current is in a delicate balance. A slight increase accelerates growth, but if exceeded, growth is retarded. Electroculture would work consistently if the electrical fields were controlled to produce the correct current in each plant.

The measured electrical characteristics permit a rational explanation for most of the reported interactions between humans and plants.

DETERMINATION OF BACTERIAL SUSCEPTIBILITY TO EXPERIMENTAL CHEMOTHERAPEUTIC AGENTS

John C. Foltz

Mt. Vernon High School, Alexandria, Va.

Purpose.—To determine the effectiveness of 2 research antibiotics against 4 laboratory strains of pathogenic bacteria.

Procedure.—Applied antibiotic impregnated discs to media swabbed with innoculum of each of the following bacteria: 1) *Streptococcus pyogenes*, 2) *Salmonella typhimurium*, 3) *Staphylococcus aureus*, 4) *Escherichia coli*. Used both experimental research drugs (Cefazolin Sodium and Tobramycin), against all 4 strains of bacteria. Placed plates in incubator set at 37° C. Examined plates after 20–24 hours of incubation and measured zones of inhibition of microbial growth as an indication of antimicrobial effectiveness.

Conclusions.—To determine the effectiveness of these 2 experimental antibiotics, I first had to determine the effectiveness of commercially available antimicrobial agents against the same 4 strains of bacteria. The standard way of differentiating the susceptibility of bacteria to antibiotic impregnated discs is through the use of predetermined zones of inhibition.

Based on the 400 tests I made with the standard drugs, I was able to judge the effectiveness of them against these bacteria. Along with these series, I also conducted 80 trials with the experimental antibiotics.

On the basis of my observation, I formulated a tentative zone of inhibition for each antibiotic. Against organisms known to be gram-positive, the experimental chemotherapeutic agent Tobramycin should have a zone of inhibition of 19 mm. to be called sensitive. For gram-negative organisms against the research antibiotic Cefazolin Sodium, the zone of inhibition should be equal to or exceed 24 mm. For the experimental drug Tobramycin against known gram-negative organisms, the inhibition zone size should be at least 18 mm. to be sensitive.

Up to this point, the same antibiotic showed rather consistant sensitivity reactions against both negative and positive pathogenic organisms. However, one significant exception was noted: Cefazolin Sodium produced an inhibition zone of 49 mm. against gram-positive *Streptococcus pyogenes* and a zone of only 32.8 mm. against gram-positive *Staphylococcus aureus*. Therefore, *Streptococcus pyogenes* is seemingly sensitive, while *Staphylococcus aureus* appears to be resistant.

EFFECTS OF BLOOD TRANSFUSIONS ON ANTIBODY FORMATION AND CANINE RENAL ALLOGRAFT SURVIVAL

Jandel Theresa Allen

Immaculata Preparatory School, Washington, D.C.

Kidney disease, and related renal complications, have fascinated as well as plagued medical science for many years. When it was discovered that compatibility among donor and recipient made it possible for an increase in survival rate of nearly 100%, there was finally hope for patients suffering from acute renal complications. However, the search for a compatible donor is often hard, tedious, and sometimes fruitless work. Either a suitable organ is not available, or, as is quite realistic, a compatible donor may not be willing to donate the kidney for various reasons. Therefore, scientists began to search for alternatives—some means of transplanting incompatible kidneys—while at the same time administering some drug that will both increase survival while it decreases rejection. Is it possible to transplant incompatible organs and use various drugs, or combinations of drugs, to increase survival? What effect do blood transfusions administered to patients on hemodialysis have on renal allograft prolongation? And from that point, what effect do various methods of preparing blood transfusions have on renal allograft survival, if any? These questions, along with many others, are constantly asked and explored by researchers. While an ideal method—that is, one that will increase survival rate to 100%—has yet to become a reality, science gets closer to an answer with each passing experiment.

In discussion of blood transfusions as a possible means, one main factor must be considered, and this is the composition of blood. Blood consists of red blood cells, white blood cells, and plasma. It is the white cells, or leukocytes, we must be concerned with when discussing transfusion and their effects in renal allograft survival, and more important, antibody formation. When foreign bodies of any form or fashion enter the conditioned internal environment, these leukocytes immediately go to work to form antibodies that will combat these objects. These antibodies will then set out to reject the foreign object. This tends to throw off the body's metabolism. If one could find some possible way of controlling the formation of these antibodies, we would be a step closer to a solution to the rejection problem.

This paper seeks to show how, by means of blood transfusions, renal allograft survival is, in fact, increased and how blood prepared in a variety of methods further increases this rate.

(This work was performed as part of a team at Washington Hospital Center as a Washington Heart Association summer research student.)

THE ANALYSIS OF TAP WATER SAMPLES FOR DDT AND ITS ISOMERS THROUGH USE OF THE GAS CHROMATOGRAPH

Stephany DeScisciolo

Washington-Lee High School, Arlington, Va.

This summer I was given the opportunity to work in a chemical laboratory through the Summer Student Research Program sponsored by the National Science Foundation. The purpose of my experiment was to determine whether or not area water treatment plants have

an efficient way of ridding the raw water of any DDT, DDD, or DDE residue. To do this, samples of water had to be collected from the water treatment plant before treatment and samples after all treatments for drinking water had been completed. Then the samples had to be prepared for the gas chromatograph (GC), run on the GC, and the results calculated in terms of what kind of pesticide and how much was present in the sample. Three area water treatment plants were contacted, and samples were collected from two of them—Lorton Treatment Facilities on the Occoquan River, and Dalecarlia Water Plant on the Potomac River.

The samples were collected in acid-washed, 1-liter bottles. The first water plant from which samples were collected was Lorton Treatment Facilities. Two l treated water, and 2 l raw, untreated water were collected from the plant. The samples were then taken back to the lab, and preparations for the gas chromatograph were begun immediately. The raw water was filtered, and the particulates from each of the 2 samples of raw water were treated as separate samples. To extract the filter paper, a Soxhlet extractor was set up and the paper extracted for 24 hr. Two blanks consisting of distilled water were also prepared. The prepared samples were then injected into the gas chromatograph. As soon as all samples had been run, a calculation of the results was started. After calculating the results of the GC tracing, I found evidence of p,p′-DDE, p,p′-DDT, o,p′-DDT, and p,p′-DDD in the samples collected from Lorton.

The next step was to prepare standards of each of the previously named pesticides so that quantitative results could be found. Standards of 10 ppb, 100 ppb, and 300 ppb were prepared for each of the pesticides found in the samples. After the standards were run through the gas chromatograph, quantitative analysis was begun. The samples of raw water (particulates and liquid treated as one sample in reporting results) taken from the Lorton Treatment Facilities yielded .1565 ppb of p,p′ DDE, .1765 ppb p,p′-DDT, .2745 ppb o,p′-DDT, and .028 ppb p,p′-DDD. (The above numbers are averages of the two samples of raw water taken). In the treated water from Lorton, there was no evidence of DDT, DDE, or DDD. As I am still calculating the results obtained from the water samples collected from Dalecarlia Water Plant, I do not have sufficient results to report.

After completing the study of the samples taken from Lorton Treatment Facilities, I conclude that this particular water treatment plant has an efficient method of removing DDT and its isomers from raw, untreated water.

LEUKEMIA— A COMPREHENSIVE REPORT

Robert Kucbel

McLean High School, McLean, Va.

Leukemia means white blood, which is the name that was given to this disease when it was first discovered in 1847. The name is misleading however, as leukemia is a cancer of the tissues in which blood is formed, mainly the bone marrow, the lymph nodes, and the spleen.

Leukemia is worldwide in distribution, and during the second quarter of this century, the incidence of this disease more than doubled. Altogether, the different types of leukemia are responsible (either directly or indirectly) for 15–19,000 deaths annually in the United States. The incidence of leukemia is greater in males than in females, but the increase is prevalent in both sexes, especially in the older age group.

In 90% of all detected leukemia, 2 types—granulocytic and lymphocytic

—and 2 recognized clinical forms—acute and chronic—are involved. The remaining 10% are mainly cases of acute monocytic leukemia.

In the acute leukemias, primitive and undifferentiated leukocytes are produced and discharged into the blood. They have an irregular structure, they fail to mature, and upon division, they may produce 3 or more daughter cells instead of the normal 2.

In chronic leukemia, the leukemic cells are well differentiated and mature, as well as being quite similar in many aspects to their normal counterparts. The distinguishing abnormal characteristics are found in the chromosomes and in the cell metabolism.

Among the many causes of leukemia are genetic factors, chemicals (although proof is still scant), radiation, and viruses.

Symptoms of acute leukemia may include lack of energy, fatigue, headache, persistent sore throat, loss of appetite and weight, pallor, anemia, shortness of breath, and recurrent infections. If leukemia is not detected at this stage, it may suddenly manifest itself with fever, severe fatigue, and bleeding disturbances. Chronic leukemia follows the same course, only it occurs in a span of 2–10 years before hemorrhaging and infection occur.

Twenty years ago, a leukemic child lived only 3 months after diagnosis. Today most youngsters live 2–5 years, and in some cases even longer after diagnosis. Treatments include drug therapy, antimetabolites, alkylating agents, antibiotics, plant alkaloids, and L-asparaginase (the first use of an enzyme in cancer chemotherapy).

Scientists are working on methods to totally kill the leukemic cell. This is a concentrated and time-consuming effort in which there are many dead ends, but it will be the key that will unlock more mysteries on the function of chemicals in the human body. This key will also provide a new insight to biology, or the need for biology to understand nature's processes.

COLI/STREP RATIOS IN SPOUT RUN

John Maloney

Washington-Lee High School, Arlington, Va.

The purpose of this project was to attempt to determine whether the high coliform counts in Spout Run can be attributed to contamination from the sanitary sewers which run parallel to the stream (as well as the storm sewers feeding it), or to contamination by the water which runs off the streets which could be fouled by the wastes of small mammals (cats, dogs, squirrels, etc.). A series of 2-part bacterial counts, one, a fecal coliform (indicator of human fecal wastes) count, and the other, a fecal *streptococcus* (indicator of small animal wastes) count, has been suggested as a tentative test to determine whether waste material in a stream is of animal or human origin. The ratio of the coliform count to the *Streptococcus* count would indicate the kind of fecal material which was present in the stream. A ratio of 4.4:1, coliform to *Streptococcus*, has been suggested as being very strong evidence of human fecal material in the stream.

Using the membrane filter technique I plotted the counts on a graph. These counts seem to indicate that, during certain times of the year at least, there is human waste material present in the stream. This would indicate that the sanitary sewers are leaking and/or seeping into Spout Run and that some action should be taken by Arlington County's Sanitation Department to correct this situation.

Acknowledgment.—I would like to thank the Arlington County Depts. of

Highways and Sanitation for providing me with maps of the sewer lines. A special vote of thanks goes to the Arlington Environmental Improvement Commission for their help and for providing the material and equipment which made this project possible.

AIR POLLUTANTS AND MORTALITY IN WASHINGTON, D.C.— A STATISTICAL ANALYSIS

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Although scientific investigations have shown some high pollutant concentrations to be detrimental to health, very little is known about the actual effect on mortality of pollutants in ambient city air. For instance, Washington air pollution measurements have been recorded for several years and air pollution alerts have been issued, but Mr. David DiJulio, Air Quality Program Manager of the Metropolitan Washington Council of Governments, informed me that this study was the first ever done on the relationship between air pollutants and mortality in Washington.

The relationships between mortality and Washington pollutant levels of NO_2 (nitrogen dioxide), O_3 (ozone), and CO (carbon monoxide) were investigated in this study. Possible associations were examined by the use of scatter plots and regression analysis. The results indicate, among other things, that NO_2 has a negative correlation with mortality, O_3 is not correlated with mortality, and CO has a significant positive correlation with mortality. The NO_2 result may be attributed to a negative correlation between NO_2 and CO. According to Dr. Wilson Riggan, the director of the E.P.A. Human Effects Laboratories in Durham, N.C., the CO results provided evidence for the first time of the suspected association between mortality and ambient CO levels of a city and gave added justification for the E.P.A. national standards. This relationship was pronounced in Washington because CO levels are unusually high—they frequently exceed national standards.

It is hoped that many more studies of this type will be done for urban areas that have different pollutant mixtures and weather conditions so that national standards can be established with adequate confidence that the air will be safe to breathe.

THE ULTRASTRUCTURE OF VIRAL NUCLEIC ACIDS

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This study involved the nature of viral nucleic acids, RNA or DNA. The viruses studied include those containing single-stranded RNA, principally, vesicular stomatitus virus (VSV) and various RNA tumor virus. The control molecules were the single-stranded DNA containing bacteriophage ØX 174. Two methods were used to study the purified nucleic acids. The first method is a technique which allows for purified RNA and DNA molecules to be seen with the electron microscope. This allowed for an analysis of molecule length and conformation. The second method is a way of biochemically confirming the results found with the electron microscope, i.e. by velocity sedimentation centrifugation. This latter method allows for the analysis or separation of different molecules on the basis of size, by centrifugation through a gradient of sucrose or glycerol.

The first technique involves the preparation of grids to be viewed with the electron microscope using the methods

of Kleinschmidt and Zahn (1959). The 2 spreading conditions are the aqueous spread which contains no denaturing agent and the urea formamide spread which contains the denaturing agents urea and formamide. RNA or DNA molecules which are double-stranded will appear the same under both conditions, but those molecules which are single-stranded will appear clumped under aqueous spreading conditions and extended under the urea formamide spreading conditions. The single-stranded molecules are not extended without denaturing conditions because they have numerous intramolecular bonds. Thus not only the nature of the molecules but also the measurement of their length can be determined after spreading them under both conditions.

The second method of analyzing the nucleic acid molecules is by velocity sedimentation centrifugation. The RNA is radioactively labeled with tritium and applied to a 10–30% (v/v) glycerol gradient. After centrifugation, fractions are collected and samples from each fraction are taken and counted in a Packard scintillation counter to measure the amount of radioactivity. The activity of each fraction can be analyzed and the size of the molecules calculated.

These techniques are important for determining the length and size of the DNA and RNA molecules. They were used also to study the nature of the RNA from a mutant of USV thought to have double-stranded RNA as its genome. The results of the 2 techniques complement each other.

A Preliminary Annotated Bibliography of Information Handling Activities in Biology

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ABSTRACT

A selected bibliography containing about 300 references to information handling activities in the biological sciences is presented. Each reference is annotated to indicate subject matter. The bibliography generally excludes a) articles that are limited to *Homo sapiens* and his disorders, b) many articles published before 1965, c) general texts and papers not specifically related to biological subjects, and d) most references not in the mainstream of the biological literature.

During the past several years biologists have awakened to the need for finding better ways to handle the rapidly growing amount of information they use and generate. This need has been expressed by a few, but the lack of concerted action in this direction has been an ever-growing source of concern to many biologists.

The disciplines of biology vary so greatly in their subject matter and methodology that a centralized effort to establish a truly comprehensive information "system" in biology may never succeed. Nevertheless, progress toward solving information handling problems has advanced in varying degrees from discipline to discipline, and in some

cases it is evident that quite satisfactory solutions are well on the way toward being made. In the large picture, BIOSIS of Biological Abstracts has made impressive progress toward the control of the biological literature, but other areas of activity have hardly been touched upon by biological scientists.

In a very real way the following bibliography represents a recorded summary of progress made to date by the biological community in various areas of information handling. It is intended to indicate the outstanding specific efforts made in several biological disciplines, and it presents sources of information about the more comprehensive activities in biology. At the same time it emphasizes those areas in which more concerted activity seems to be indicated (e.g., the use of microform by biologists).

Exclusions

The user of this bibliography should be aware of its limitations:

1. *References dealing largely or exclusively with Homo sapiens and his origins, development and disorders* have been excluded. However, an occasional reference in this subject matter area is included because a described system or project may provide information applicable to organisms other than humans (see Eichhorn and Reinecke, 1970, concerning the Vision Information Center, which is known to deal in part with information concerning vision in insects and other animals).

2. *In large part, references to general works, including textbooks, that do not deal specifically with the subject matter of biology*. These include books and papers dealing with the principles governing the generation, use, and effective handling of information not related to a specific biological discipline. (In excluding such works, we recognize that we may be doing an injustice to the reader, but this immense body of literature has been or is being covered elsewhere and is beyond the scope of the present work.)

3. *References that might be difficult for biologists to obtain or use*. This includes most foreign-language publications; most articles that appear solely in the literature of information science; and many notes, comments, published letters, addenda, etc., relating to specific biological subjects that are hardly understandable without reference to some larger, more comprehensive work.

4. *Many articles on the subject that were published before 1965*, a date we regard as a turning point in information handling activity in biology.

Almost no bibliography, no matter what the subject, escapes the shortcoming of failing to include everything of significance. This collection is no exception. Wherever we have excluded a significant reference by oversight, we tender our sincere apologies. Where we have deliberately excluded an article or area of activity, we hope the users of this article will argue their point with us privately or in the press. In either case, an effective future revision of this compilation will depend largely on your comments and cooperation.

Subject Matter Classification and Annotations

Each entry in the following bibliography is annotated in accordance with the following subject matter classification:

A. General

1. State-of-the-art, problems, need for improvement.
2. Descriptions of broadly based (organizational) information efforts.
3. General texts.

B. Primary publications

1. State-of-the-art, problems, need for improvement.
2. Surveys of primary publications

by discipline, descriptions of core literature.

C. Secondary literature information activities

   1. Description of primary-secondary relationships and need for improvement.
   2. Cataloging and indexing, including discussions of indexing terms, subject headings, thesauri, etc.
   3. Abstracts.
   4. Descriptions of secondary systems, subject-matter content, methodology, critiques.

D. Data information systems

   1. Descriptions of systems, subject-matter content, methodology, critiques.
   2. Descriptions of computer programs.
   3. Management of collection, museum, and specimen data.
   4. Surveys, automated mapping procedures.
   5. Automated identification procedures.
   6. Automated catalogs, taxonomic catalogs.
   7. Bionumeric codes.

E. Personal information systems

   1. Mechanical.
   2. Automated.

A section following the bibliography accumulates all of the references within each of the subject-matter categories listed above.

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A. General

1. State-of-the-art, problems, need for improvement: Anon. 1974; Arnett 1970b, d, 1972; Baker 1970; Baker *et al.* 1972; BIOSIS 1965, 1970; Brodo 1971; Egle 1973; Favorite 1964; Foote 1967, 1970, 1972b; Foote & Hammack 1969; Heumann 1974; Gordon 1969; Hattery 1961; Herman 1973; Kendrick 1964; Mello 1974; NAS 1970; Parkins & Kennedy 1971; Russell 1962; SATCOM 1969; Shetler 1973, 1974; Shetler & Krauss 1971; Smith 1970; Steere 1970; Woodford 1969.

2. Descriptions of broadly based (organizational) information efforts: Anon. 1954, 1970c, 1972; Atz 1968; Baker 1970; Bejuki 1965; BIOSIS 1970; Burton 1969; Egle 1973; Foote 1967, 1969, 1972a; Foote & Hammack 1969; Fowler 1965; Gates 1971; Graham & Foote 1971; Heumann 1974; Irwin 1973; Kiehl 1970; Krauss 1973b; Mohler 1969, 1970; Smith 1970; Walsh 1973; Wise 1973.

3. General texts: Arnett 1970d; Bottle & Wyatt 1967; Morse, Furlow & Beaman 1971.

B. Primary publications

1. State-of-the-art, problems, need for improvement: Anderson & Van Gelder 1970; Bamford 1972; Brodo 1971; Brown 1961; Brown *et al.* 1967; Conrad 1965; Davis 1973; Foote 1967; Garfield 1964; Kennedy & Parkins 1969; Lamanna 1970; Lewin 1971; Mohler 1972; NAS 1970; Parkins 1971; Porter 1967; Randal & Scott 1967; Walker 1965; Wolf 1966; Wooster 1970; Yochelson, 1969; Zweimer 1970.

2. Surveys of primary publications by discipline, descriptions of core literature: Alvorson 1964; Anderson 1966; Anderson & Van Gelder 1970; Anon. 1967; Baldwin & Oehlerts 1964; Brigham 1974; Brown 1956; Brygoo 1965; Conrad 1965; Dimond 1970; Foote & Hammack 1969; Gorham 1968; Gurtowski 1968, 1970; Hahn 1973; Hammack 1970b; Heumann 1974; ICSU-AB 1967; Kull 1965; Lentz 1969; Mello 1969; Norris 1971; Packer & Murdoch 1974; Shilling & Benton 1964; Simon 1970; Smith & Reid 1972; Trauger *et al.* 1974; Tunevall 1969.

C. Secondary literature information activities

1. Description of primary-secondary relationships and need for improvement: Anderson & Van Gelder 1970; Arnett 1969b; Baker 1970; Bamford 1972; BIOSIS 1970; Brodo 1971; Brown *et al.* 1967; Crovello & MacDonald 1970; Edwards 1971a; Foote 1967; Foote & Hammack 1967; Gordon 1972; Kennedy & Parkins 1969; Morgans 1965a; NAS 1970; Parkins 1971, 1974; Parkins & Kennedy 1971; Shervis *et al.* 1972; Wood *et al.* 1972, 1973; Zweimer 1970.

2. Cataloging and indexing, including discussions of indexing terms, subject headings, thesauri, etc.: Anderson 1962; Anon 1963; Arnett 1971; Bean 1969; BIOSIS 1970, 1973; Garfield 1964; Herting 1964; Schultz 1968; Shervis & Shenefelt 1973a, b; Shervis *et al.* 1972; Travis *et al.* 1962; UNESCO 1970.

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D. Data information systems

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MacDonald & Reed 1968; Manning 1969a; McAllister *et al.* 1972; Meadow 1970; Morgans 1965b; Morse 1974a; NAS 1970; Noyce 1965; Perring 1971b; Peters 1970; Radford & Pankhurst 1973; Randal & Scott 1967; Reddin & Feinberg 1973; Rogers 1966; Rogers *et al.* 1967; Savage 1964; Shetler 1971, 1973, 1974; Shetler *et al.* 1969; Shetler & Krauss 1971; Shetler *et al.* 1973; Shetler & Read 1973; Skerman 1973; Squires 1970; Suzynski 1971; Taylor 1971; Turnbull 1967; Van Gelder & Anderson 1967; Vance 1970; Wade 1972; Walker *et al.* 1968; Walters 1963; White & Grodhaus 1972; Whitehead 1971; Wood 1954; Wood *et al.* 1963; Yerke 1971.

2. Descriptions of computer programs: Addison *et al.* 1969; Anon. 1970b; Bachmann *et al.* 1973; Burton 1969; Chenhall 1972, 1975; Creighton & Crockett 1971; Creighton & King 1969b; Creighton & Packard 1974; Creighton *et al.* 1972; Cutbill 1971; Haglind *et al.* 1969; Hudson *et al.* 1971; Hull *et al.* 1970; Krauss 1973a; Pankhurst 1970b; Reddin & Feinberg 1973; Rickman *et al.* 1972.

3. Management of collection, museum, and specimen data: Albrecht & Skavanl 1974; Anon. 1970b, [date?]; Anderson 1962; Argus & Sheard 1972; Arnett 1969a; Beamen 1971; Berry 1970; Beschel & Soper 1970; Brenan 1974; Chenhall 1974, 1975; Creighton & Crockett 1971; Creighton & King 1969a: Creighton & Packard 1974; Crovello 1967, 1972a; Crovello *et al.* 1970; Cutbill *et al.* 1971; Cutbill & Williams 1971; Gomez-Pompa & Nevling 1973; Greene 1972; Hall 1972a, b, 1974; Heath 1971a, b; Johnson *et al.* 1971; King *et al.* 1967; Landrum 1969; Lewis 1967; MacDonald 1971; Manning 1969a; McAllister *et al.* 1972; Meikle 1971; Mello & Collier 1972; Morse 1974a; Perring 1963, 1967, 1971a; Reed *et al.* 1963; Shetler 1973, 1974; Shetler *et al.* 1973; Soper 1969; Soper & Perring 1967; Squires 1966, 1968, 1971; Suszynski 1971; Vance 1970; Walker *et al.* 1968.

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6. Automated catalogs, taxonomic catalogs: Arnett 1970c; Chenhall 1973, 1974, 1975; Krombein *et al.* 1974; Manning 1969b; Meikle 1971.

7. Bionumeric codes: Bullis & Roe 1967; Denmark *et al.* 1958; Gould 1954; Hull 1966; Jahn 1961; King *et al.* 1967; Little 1964; Manning 1969a; Michener 1963; Mullins & Nickerson 1951; Rabel 1940; Reed *et al.* 1963; Rivas 1965.

E. Personal information systems

1. Mechanical: Baker 1970; Brindley & Jones 1969; Bryan 1966; Byer *et al.* 1959; Duke 1969; Gould 1958; Levine 1955; Lloyd 1969; Morgans 1965a; Perdue 1964; Reichl 1963; Reinecke 1967; Shenefelt 1969; Van Gelder & Anderson 1967; Walters 1963; Wilcox 1968; Wood *et al.* 1963.

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Amblycerus acapulcensis, A New Species of Seed Beetle from Mexico (Coleoptera: Bruchidae)

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ABSTRACT

A new species of bruchid, *Amblycerus acapulcensis*, that feeds in seeds of a leguminous tree, *Caesalpinia cacalaco*, in Mexico is described and distinguished from other species in the genus.

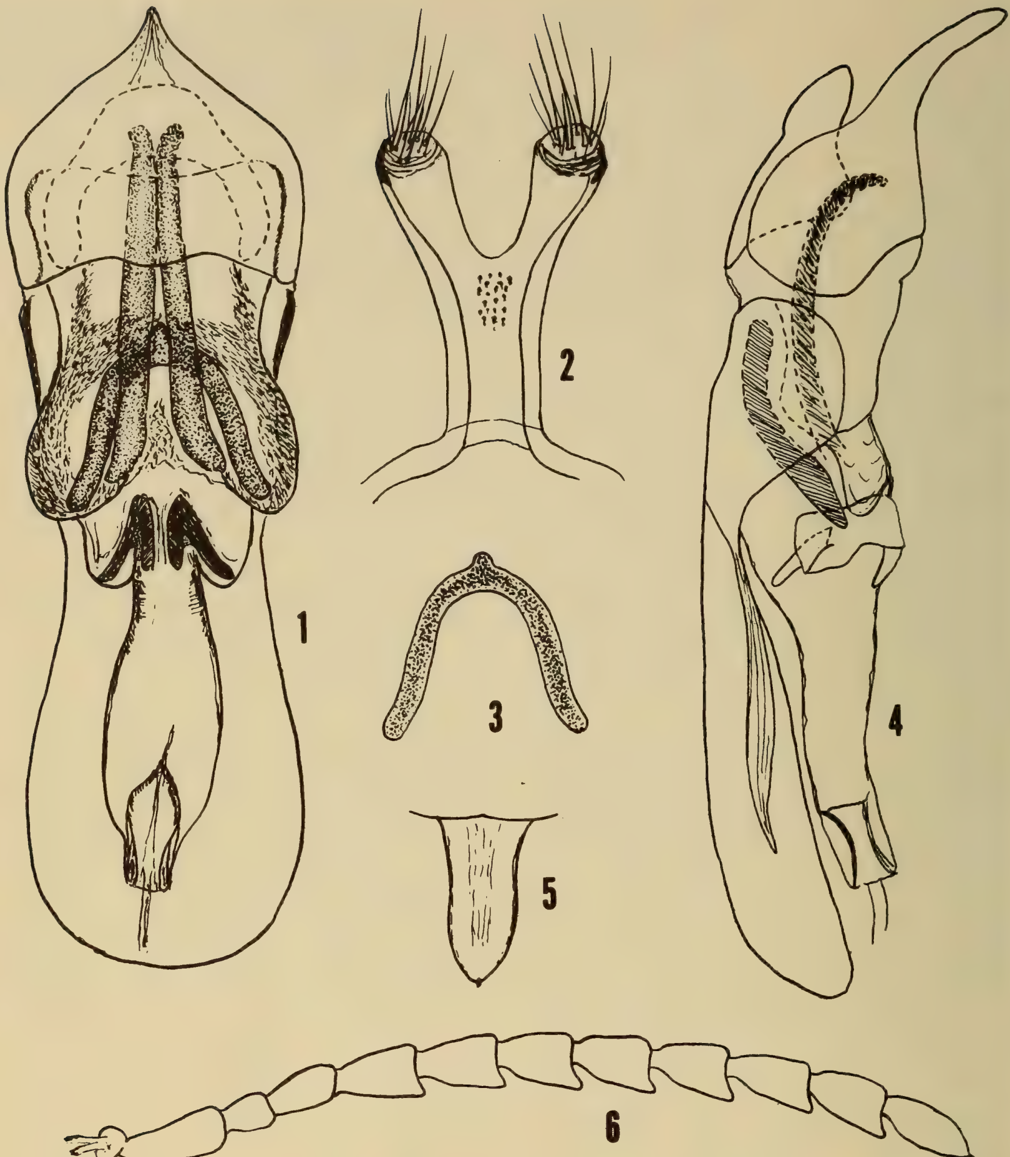
To provide a name for a bruchid involved in biological studies currently underway, this description is presented.

Amblycerus acapulcensis Kingsolver, n. sp.

Male and female similar in external characteristics except for pygidium (see below). With characters of genus as given by Kingsolver, 1970b, p. 471. Color: Integument dark red throughout. Vestiture very fine gray and golden hairs intermixed in faintly mottled pattern. Surface of body with small, black, setiferous pits especially noticeable on elytra. Pygidium with darker median area.

Body elliptical in dorsal aspect, arched strongly in lateral aspect. Head turbinate, eyes coarsely faceted, shallowly emarginate, strongly bulging laterally; frons evenly convex, finely punctulate, with a faint median carina in lower half; clypeus slightly more coarsely punctulate than frons; labrum nearly impunctate; vestiture dense except for a pair of transverse bare spots on frons at level of dorsal margin of eyes; antenna with first segment as long as width of frons between eyes, remaining segments with proportions as in fig. 6; antenna reaching anterior margin of hind coxa. Pronotum nearly semicircular, weakly convex in basal half, more strongly convex toward apex, surface without asperities except for a pair of subbasal depressions and remnants of the submarginal sulcus either side of basal lobe; disk punctulate with fine foveolae scattered over surface, some of which are pigmented, lateral margin not carinate, abruptly rounded and with a fine submarginal sulcus on concave pleural surface, this connected with a short, vertical, submarginal sulcus laterad of anterior foramen; prosternum flat, constricted between coxae, precoxal margin with a fine submarginal carina. Elytra evenly convex, striae regular, only slightly impressed, strial punctures fine, elongate, approximate; elytral apices evenly rounded; scutellum (fig. 5) elongate-cordate, apex with minute nipple; mesosternum strap-like, rounded apically; metasternum with slight depression in caudal half, depression divided by deep, narrow sulcus, postcoxal sulcus interrupted on intercoxal lobe, parasutural sulcus of metasternum extending three-fourths distance to posterior margin; metepisternum sparsely punctate with transverse anterior sulcus deep but with parasutural sulcus nearly obsolete; face of hind coxa sparsely punctate on lateral three-fourths, and with dense cluster of punctures near trochanteral insertion; hind tibial calcar half as long as basitarsus, inner calcar half as long as outer calcar. Abdominal sterna without unusual modification; pygidium of male with terminal margin evenly rounded, that of female bisinuate.

Male genitalia.—Median lobe (figs. 1, 4) somewhat depressed, about three times as long as wide; ventral valve ogival in ventral aspect, apex arcuate in lateral aspect with base bent dorsad on either side to enclose dorsal valve base; dorsal valve U-shaped with anterior arms somewhat elongated; armature of internal sac consisting of two elongate,



Amblycerus acapulcensis. Male genitalia: Fig. 1, median lobe, ventral; fig. 2, lateral lobes, ventral; fig. 3, U-shaped sclerite of internal sac; fig. 4, median lobe, lateral. Fig. 5, scutellum. Fig. 6, antenna.

curved processes in basal half of sac, a U-shaped sclerite (fig. 3) with a short nipple on the bend, a complex of slender sclerites in middle of sac, and a terminal complex of thin, rodlike sclerites. Lateral lobes (fig. 2) setiferous, rounded apically, the cleft between them deeply rounded.

Holotype male, Mexico: Veracruz, Cerro Gordo, Riconada, April 1969, G. B. Vogt, coll., in seeds of *Caesalpinia cacalaco* Humb. & Bonpl. USNM Type #72812. Allotype female and 22 male and 25 female paratypes, same data. Other paratypes as follows, all from Mexico: Veracruz, Cerro Gordo (no date), C. L. Gilly, in *Caesalpinia cacalaco*; Sinaloa, 6 mi. N. Los Mochis, 24-II-1973, C. D. Johnson coll. #181-73, in *Caesalpinia cacalaco*; same locality and host but 12-III-1973, C. D. John-

son coll. #508-73; Sinaloa, 4 mi. N. Guamuchil, 13-VII-1968, C. D. Johnson coll. #308-68, in *Caesalpinia cacalaco*; Sinaloa, Mazatlan, 3-V-1935, in seeds of *Caesalpinia* sp.; Sinaloa, 7 mi. N. Mazatlan, 11-III-1973, C. D. Johnson coll. #489-73, in *Caesalpinia cacalaco*; Sinaloa, 10 mi. S.E. Guamuchil, 12-III-1973, C. D. Johnson coll. #506-73, in *Caesalpinia cacalaco*; Sinaloa, 26 mi. S. Culiacan, 25-II-1973, C. D. Johnson coll. #189-73, in *Caesalpinia cacalaco*; Sinalao, 4 mi. S. Culiacan, 25-II-1973, C. D. Johnson coll. #184-73, in *Caesalpinia cacalaco*; Colima, 10 mi. N. Colima, 6-III-1973, C. D. Johnson coll. #367-73, in *Caesalpinia cacalaco*; Colima, 3 mi. S. Colima, ca 1500′, 7-III-1973, C. D. Johnson coll. #374-73, in *Caesalpinia cacalaco*; Guerrero, Acapulco, 1903, E. W. Nelson Botanical coll. #6975; Oaxaca, Oaxaca, 26-XI-1958. Paratypes are deposited in collections of Northern Arizona University, Flagstaff; Canadian National Collections, Ottawa, Ontario; U. S. National Museum of Natural History, Washington, D. C.; British Museum (N. H.), London.

Amblycerus acapulcensis is closely related to *A. robiniae* (F.) which develops in seeds of *Gleditsia triacanthos* L., or honey locust, in the eastern half of the United States, and *A. taeniatus* (Suffrian) which develops in seeds of *Caesalpinia bijuga* Swartz in Cuba. Genitalia are illustrated for the latter two bruchids (Kingsolver 1970a and 1970b, respectively).

External differences among the 3 species in this group are rather subtle. The black setiferous pits on the surface of the elytra are more conspicuous in *robiniae* and *taeniatus* than in *acapulcensis*, and the pygidium has the median area piceous in *robiniae* and *acapulcensis*, unicolorous in *taeniatus*.

Group characters for the Robiniae Group are: scutellum elongate-cordate (Kingsolver 1970b, fig. 12, scutellum for *taeniatus* is too broad); apical margin of pygidium in female bisinuate, in male evenly rounded; surface of body with prominent, setiferous, black pits; male genitalia with characteristic form. Characters in the male genitalia are used to separate the 3 species in the following key:

1. Internal sac without a pair of elongate median sclerites overlapping median U-shaped sclerite; U-shaped sclerite without nipple at bend of U; apices of lateral lobes truncated, the cleft between V-shaped and more shallow .... *robiniae* (F.)
   Internal sac with a pair of elongate sclerites; U-shaped sclerite either with nipple or constricted in bend of U; apices of lateral lobes truncated or rounded but always separated by a deep cleft 2
2. U-shaped sclerite with nipple at bend (fig. 3); dorsal valve with anterior arms elongated; apices of lateral lobes rounded.................. *acapulcensis* n. sp.
   U-shaped sclerite without nipple at bend, but nearly divided into two halves; dorsal valve with anterior arms short, diagonally truncated; apices of lateral lobes truncate (Cuba only) .... *taeniatus* (Suffrian)

Caesalpinia cacalaco is listed by Stanley in his *Trees and Shrubs of Mexico* (1922) as a source of tannin and a black dye similar to that produced by *Caesalpinia coriaria* (Jacq.) Willd.

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Species of *Conotrachelus* Schönherr and *Microscapus* Lima (Coleoptera: Curculionidae: Cryptorhynchinae) Associated with *Hymenaea courbaril* Linnaeus in Central America, with Notes on the Cristatus Group of *Conotrachelus*

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ABSTRACT

The Cristatus Group of *Conotrachelus* is defined and its 9 included species keyed; natural history data are given for 3 of these species. *Conotrachelus boucheri* Whitehead is a new species of the Cristatus Group, described from specimens extracted from pods of *Hymenaea courbaril* in the Osa Peninsula of Costa Rica. Locality records are given for *Microscapus hymenaeae* Lima, based on specimens collected in association with *Hymenaea* in Panama, Venezuela, Bolivia, and Brazil.

Members of 3 weevil genera, all Cryptorhynchinae, have previously been reported in association with fruits of the legume tree *Hymenaea courbaril* L. and other *Hymenaea* spp. (Silva *et al.* 1968): *Metoposoma* Faust, *Microscapus* Lima, and *Rhinochenus* Lucas. During the course of ecological studies on *Hymenaea courbaril* in Central America and northern South America, D. H. Janzen, of the University of Michigan, has accumulated a wealth of fresh material. Included are large numbers of several species of *Rhinochenus*, which I will treat separately (Whitehead mss.), and small series each of *Microscapus hymenaeae* Lima and a new species of *Conotrachelus* Schönherr as reported below. In order to place the new species of *Conotrachelus* in some meaningful systematic context, I establish herein, as a group of convenience, the Cristatus Group of *Conotrachelus*, include a key to known species, and indicate available ecological data.

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Terminology.—In general I follow accepted terminology as heretofore used in studies on Curculionidae, but explanations are needed for the various terms used to describe integumental microsculpture. I use the term "isodiametric" if the meshes of the microsculpture form a flat honeycomb effect, "granulose" if the meshes are similar in form but tuberculose rather than flattened, or "stretched" if the meshes are clearly elongated. The arrangement of the "stretched" meshes on the pronotum and elytra is roughly transverse on average, and on the pronotum it matches the general orientation of punctations, rugosities, and other macrosculpture.

The Cristatus Group of *Conotrachelus*

Diagnostic combination.—Several species of *Conotrachelus* form an easily distinguished though not necessarily natural group, having the following characteristics in combination:

Apex of each elytron with conspicuous bare area; front coxae contiguous; femora bidentate; elytral intervals 3, 5, 7, and 9 costate, costae of intervals 3 and 5 or 3, 5, and 7 interrupted; and pronotum with lateral discal longitudinal stripes of condensed vestiture and without tubercles.

The Cristatus Group, as construed herein, may be useful for no more than recognition purposes. Similarity in external morphology may not correlate with similarity in genital morphology. I cannot, however, make broader comments on relationships without a better representation of members of the group and without first completing a thorough survey of the genus.

Key to species.—All previously described species of the Cristatus Group have been treated by Champion (1904, Central America) and Fiedler (1940, South America). I attempt in the following key to distinguish these 7 species, a new species described below, and 1 additional undescribed species. As I have seen specimens of only 6 of the 9 species, the key is based in part on the literature and hence may not prove wholly successful. I am confident of all determinations except that of *C. abdominalis*, which is not adequately distinguished in Fiedler's keys or descriptions.

Natural history.—Some natural history data are available for 3 species of the Cristatus Group. The specimens of *boucheri* were extracted from pods of *Hymenaea courbaril*. Various specimens of *cristatus* are labelled "am Licht," "am trochnem Holz," "am trochnem Holz nachts," "an welkem Laub *Hibiscus esculentus*," "at light," "banana ship," "bananas," "in brown sugar fruit fly trap," "*Inga* blüten," or "on cacao." Specimens of the undescribed species are labelled "bred from celery," "reared from Sweet Potato," or "on parsley."

1. Rostrum slender, not compressed; vestiture of dorsum white or yellow 2
   Rostrum stout, compressed; vestiture of dorsum white 6
2. Rostrum greatly elongated, in female about twice as long as head and pronotum combined and with antennal insertion near middle, in male shorter and with antennal insertion near apical ⅓; vestiture of dorsum white or yellow 3
   Rostrum shorter, antennal insertion much nearer apex in both sexes; vestiture of dorsum yellow 5
3. Elytral intervals 5 and 7 feebly costate. (No specimens seen, described from "Cayenne") .... *C. numenius* Fiedler
   Elytral intervals 5 and 7 strongly costate 4
4. Vestiture of dorsum yellow. (No specimens seen, described from Volcan de Chiriqui, Panama) .... *C. divirgatus* Champion
   Vestiture of dorsum white. (Three specimens seen, from "Guyana" and "Para") .... *C. iris* Fiedler
5. Bare areas at elytral apices contiguous, dense vestiture of sutural intervals ending near middle of bare areas. (Many specimens seen, from various localities in Brazil and Paraguay) .... *C. praeustus* Boheman
   Bare areas at elytral apices well-separated, dense vestiture of sutural intervals ending near apex of bare areas. (Four specimens seen, from Osa Peninsula, Costa Rica) .... *C. boucheri* Whitehead, NEW SPECIES
6. Elytral intervals 5 and 7 feebly costate. (No specimens seen, described from "Brazil") .... *C. obsoletus* Fiedler
   Elytral intervals 5 and 7 strongly costate 7
7. Last visible abdominal sternum coarsely punctate mesally; vestiture of middle and hind femora yellow. (Many specimens seen, from various localities in Brazil and Peru) .... *C. abdominalis* (Fabricius)
   Last visible abdominal sternum finely punctate mesally; vestiture of middle and hind femora white 8
8. Pronotum coarsely rugose, microsculpture stretched. (Many specimens seen, from various localities ranging from Mexico to Peru and northern Brazil) .... *C. cristatus* Fåhraeus
   Pronotum finely rugose, microsculpture granulose. (Many specimens seen, from various localities in Argentina and Uruguay) .... *C.* sp. nr. *cristatus*, probably undescribed

Conotrachelus boucheri Whitehead, New Species

Type-material.—Holotype female labelled "C. R. Puntarenas. nr. Rincon, Osa Peninsula. 12 March 1972. D. A. Boucher" and "ex *Hymenaea courbaril* pods CR-Osa: D. Janzen 12 March 1972". Three female paratypes, same label data. Holotype and paratypes deposited in U.S. National Museum of Natural History, Washington; USNM holotype #73362.

Diagnostic combination.—This species is distinguished from other members of the Cristatus Group by characteristics given in the key. It is most similar in appearance to *C. praeustus* but is smaller and broader as well as having clearly separated bare areas at the elytral apices.

Conotrachelus boucheri is 1 of 3 members of the Cristatus Group known to occur in Central America. Females of *boucheri*, unlike those of *divirgatus*, have the rostrum only about as long as the head and pronotum combined and with the antennal insertion near the apical ⅓. In addition to having the rostrum slender and the vestiture of the dorsum and middle and hind femora yellow, specimens of *boucheri* differ from those of *cristatus* by having the costa of interval 7 feebly rather than strongly interrupted, pronotum not rugose, and last visible abdominal sternum coarsely and densely punctate mesally.

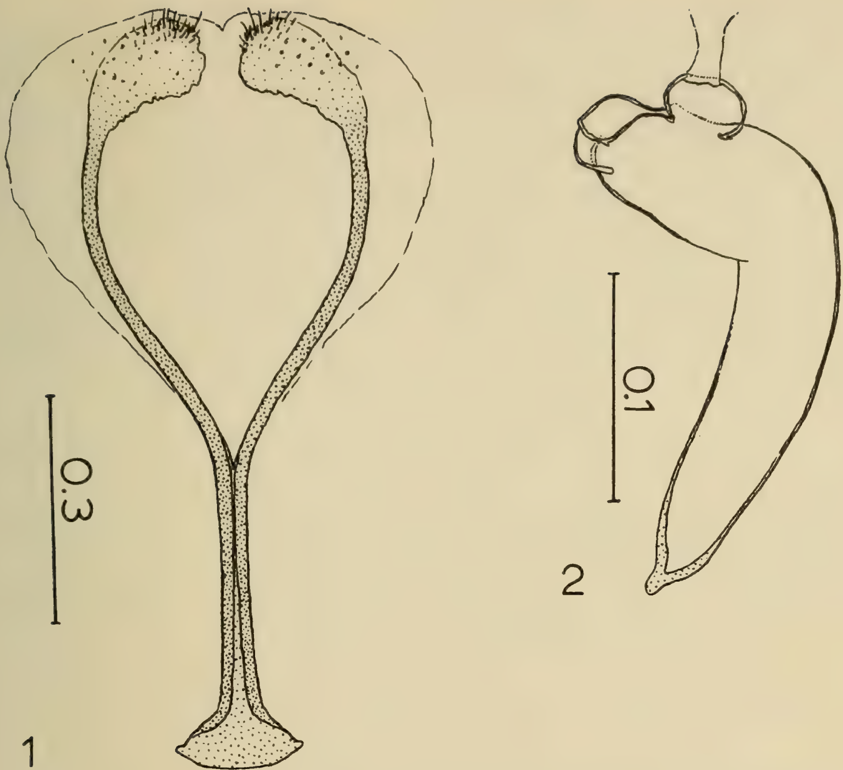
Description of female.—Length, pronotum (1.4 mm) + elytron (3.2 mm) = 4.6 mm; width of pronotum, 1.6 mm; width of elytra, 2.0 mm. Length of rostrum, base of mandible to apex of antennal insertion (0.36 mm) + apex of antennal insertion to frontal fovea (1.33 mm) = 1.69 mm; length from frontal fovea to base of pronotum, 1.67 mm. (For general statement of relative position of antennal insertion: length of rostrum, apex of mandible to apex of antennal insertion (0.47 mm) + apex of antennal insertion to eye (0.93 mm) = 1.40 mm; length from anteroventral margin of eye to base of pronotum, 1.71 mm). Maximum width of rostrum, 0.29 mm; width of head across eyes, 0.93 mm; width of frons between eyes, 0.22 mm. Length of antenna, scape (0.78 mm) + funicle (0.67 mm) + club (0.33 mm) = 1.78 mm; second funicle segment 3 times as long as wide, about ¾ as long as first.

Integument castaneous, venter rufopiceous. Vestiture recumbent, scales slender, subsetiform; pattern of dorsum about as in *cristatus* (see Champion 1904, plate 19, Fig. 9) except pronotum with discal stripes less developed and with lateral marginal pale stripe; vestiture throughout mostly of yellow to pink scales, white on posterolateral margins of pronotum and on scutellum; vestiture moderately sparse dorsally except where concentrated in pronotal stripes and along basal margin of glabrous area at elytral apex, sparse ventrally, nearly uniformly dense on dorsal surfaces of femora. *Rostrum* quadrisulcate above; median carina broad, rounded, polished; finely, sparsely punctate in apical ½; vestiture sparse in basal ½ except above eyes; width nearly uniform except for slight constriction in front of antennal insertion; depth about 1½ times greater at base than apex. *Head* densely punctate; frontal fovea deep; short, vague, longitudinal median carina extended backward from fovea. *Prothorax* above densely, coarsely punctate, punctures partially confused but surface not rugose, some fine punctures on interstices; interstitial microsculpture transversely stretched; pronotum with short, vague, median longitudinal carina in apical ½ and with shallow transverse impression in apical ¼, otherwise without distinctive tubercles or impressions; pronotum not angulate laterally, slightly wider at middle than at base; rostral canal deep. *Elytron* with strial punctures deep, each puncture with slender white scale; intervals finely tuberculate; intervals 3, 5, 7, and 9 costate; costae of intervals 3 and 5 each twice interrupted and with middle segment strongly raised; costa of interval 7 feebly interrupted behind humerus; glabrous area at apex with few, scattered, setiform scales, dense scales of sutural interval extended nearly to apex of glabrous area. *Venter* of pterothorax densely, coarsely punctate; abdomen finely punctate, last visible abdominal sternum densely, coarsely punctate throughout. *Legs* with femora bidentate ventrally, distal tooth small; distal comb of tibia orange. *Female genitalia* with spiculum ventrale and spermatheca as in Figs. 1–2.

All 4 specimens are virtually uniform in size and structure; measurements are based on the holotype.

Discussion.—I take pleasure in naming this species for D. A. Boucher, collector of the type-material.

According to D. H. Janzen (*in litt.*), ". . . if I recall correctly (these weevils) were living inside of the pods (of *Hymenaea courbaril*) and had drilled out four different seeds. On the other hand,



Female genitalia of *Conotrachelus boucheri* Whitehead; line scales in mm. Fig. 1, spiculum ventrale; fig. 2, spermatheca.

if I also recall correctly, these were in pods that had been exited by *Rhinochenus* and it may well be that they come into the pod after the *Rhinochenus* have left.'' Obviously, more field investigation is needed to work out exact ecological interactions among *Conotrachelus boucheri*, the holedriller species of *Rhinochenus*, and *Hymenaea courbaril*. Preliminary evidence, however, does suggest that *C. boucheri* is a secondary seed predator dependent on *Rhinochenus* for access to its food supply.

Also according to Janzen (*in litt.*), the weevils were collected ''within two miles of Rincon . . . from *Hymenaea* trees growing along what is commonly known as the Holdridge Ridge Trail which is roughly one mile south of the Rincon Airport and about 20 to 50 meters in elevation. It is on red lateritic hilly soil.'' There is presently some doubt that this population of *Hymenaea* is conspecific with *H. courbaril*, but Janzen thinks the Osa and Guanacaste populations are continuous.

Microscapus hymenaeae Lima

This species was described from specimens collected in fruits of *Hymenaea* sp. from 2 localities in Brazil: Cantareira, São Paulo and Santa Luzia, Minas Gerais. The original description and photographs (Lima 1950) and association with *Hymenaea* are adequate for reliable species identification. I examined 50 adult specimens from Central and South America, and found no important variation in external features or in female or male genitalia. Records cited below include the first report of this species in Central America.

Distribution records.—PANAMA. Cocle: Rio Hato, 8.I.1971, D. Wilson, from pods of *Hymenaea courbaril* (1 male). VENEZUELA. Barinas: Barrancas, 21.X.1973, D. H. Janzen, from pods of *Hymenaea courbaril* (3 adults). BOLIVIA. Beni: Rurrenabaque, XII.1921, W. M. Mann, from "Paco bean" (*Hymenaea* sp.) (37 adults, 11 immatures). BRAZIL. Goias: Ilha do Bananal, 21.IX.1926, E. G. Holt, from "fruit of Jatoba" (*Hymenaea* sp.) (9 adults).

References Cited

Champion, G. C. 1904. Biologia Centrali-Americana, Insecta, Coleoptera, Curculionidae 4: 313–440.

Fiedler, K. 1940. Monograph of the South American weevils of the genus *Conotrachelus*. British Museum, London. 365 p.

Lima, A. M. da C. 1950. Sobre alguns gorgulhos da subfamilia Cryptorhynchinae (Col. Curculionidae). Dusenia, Curitiba 1: 377–384 + 1 plate.

Silva, A. G. d'A e, *et al*. 1968. Quarto catálogo dos insetos que vivem nas plantas do Brazil. Seus parasitos e predadores. Ministerio da Agricultura, Rio de Janeiro. Volume 2: part 1, 622 + xx p; part 2, 265 p.

Euceraphis punctipennis (Zetterstedt), the Fourth Aphid Species with Four Cornicles (Hemiptera: Homoptera: Aphididae)

Louise M. Russell

Systematic Entomology Laboratory, IIBIII, Agr. Res. Serv., USDA, Beltsville, Maryland 20705

ABSTRACT

An adult *Euceraphis punctipennis* (Zetterstedt) is the first winged specimen and the first species of the aphid subfamily Drepanosiphinae to be recorded as having 4 cornicles.

A winged adult female of *Euceraphis punctipennis* (Zetterstedt) with the unusual characteristic of 4 cornicles was found among aphids collected from *Betula* sp., Mesa, Colorado, 8 June 1967, by the late F. C. Hottes. The aphid is typical in other structures, and it is the only one of the 80 individuals of the lot that exhibits a duplication of cornicles.

The adventitious cornicles are located dorsally near the body margin on abdominal segment VI, almost directly posterior to the typical pair on segment V. The posterior cornicles are virtually the same shape and length as the anterior ones, but in diameter are approximately ⅓ narrower than the anterior ones. The structure of the supplementary cornicles appears to be similar to that of the typical pair.

This example of *punctipennis* is the third winged aphid and the first species of the Drepanosiphinae recorded as having supplementary cornicles. Previously wingless examples of 3 species of the Aphidinae have been reported to have more than the 1 typical pair. In most of these insects, however, the size of the additional cornicles does not equal that of the typical ones in any dimension.

Zirnits (Fol. Zool. Hydrobiol. 2: 1–3, 1930) observed branched cornicles in

2 apterae of *Megoura viciae* Buckton. In 1 specimen both cornicles were branched, while in the other only 1 cornicle was branched but it was greatly enlarged basally. Remaudière (Rev. Path. Veg. et Entomol. France 43: 31–35, 1964) examined adults and nymphs of *Aphis* sp. with supernumerary cornicles. Of his 19 specimens, 8 had an additional cornicle on one-half of the body, 3 had an extra pair, and 8 individuals had only the usual pair. Leonard (Proc. Entomol. Soc. Wash. 68: 320, 1967) reported 1 adult aptera from several examples of *Aphis sambucifoliae* Fitch with an adventitious pair of cornicles. And Medler and Ghosh (Proc. Entomol. Soc. Wash. 69: 366, 1967) noted a winged example of *Macrosiphum* with 3 cornicles. As illustrated, the additional cornicle is nearly as large as the normal ones but all cornicles are broken in this specimen.

An alate adult female of *Aphis sambucifoliae* with an adventitious cornicle was collected as the prey of Asilidae, Baltimore County, Maryland, July 1973, by A. G. Scarbrough. The additional cornicle is located dorsally on abdominal segment VI distinctly mesad of the typical one on segment V. It is one-half the length of the typical cornicle and its greatest diameter is two-fifths the least diameter of the normal cornicle. It is slightly more slender basally than distally, whereas the normal ones are widest basally and gradually taper distally. The operculum of the abnormal cornicle appears to be replaced by a conical invagination as described by Remaudière for some of his specimens of *Aphis*.

BOARD OF MANAGERS MEETING NOTES

Oct. 17, 1974

The 627th meeting of the Board of Managers was called to order at 8:00 p.m. by President Stern in the Conference Room of the Lee Building at FASEB.

Announcements.—Dr. Stern introduced the new officers and delegates. The minutes of the previous meeting were approved as corrected.

Treasurer.—Dr. Rupp reported that $750 had been received since the last report, and that with more dues coming in, the Academy should be operating in the black. He mentioned that the Geological Society of Washington had withdrawn its request for office services from the Academy.

Policy Planning-Ways and Means.—In the absence of Dr. A. Forziati, Dr. Stern announced that, as a result of the recent overall study of the Academy, the Committee on Policy Planning had been combined with the Ways and Means Committee for the current year and that it was now composed of past presidents.

Public Information and Bicentennial Committees.—Dr. Stern also announced that Dr. J. W. Rowen is the current chairman of the Public Information Committee, and that Dr. R. J. Seeger is working on plans for the Bicentennial which are to be presented to the Board in the Spring for discussion and approval.

Meetings.—Dr. Stern announced for Dr. Honig that the meeting arrangements for the current year are all confirmed.

Encouragement of Science Talent.—Mrs. Elaine Shafrin reported that the Committee on Encouragement of Science Talent was very actively seeking 100 judges for the Saturday Science Fairs. She mentioned that if judging permits, the papers presented by students at the 2-day program, Junior Sciences and Humanities, at the Westinghouse Christmas convention, and the 6 papers to be presented by high school students at the Junior Academy will be published. [See *Features*, this issue. —*Ed*.]

Nominating Committee.—Dr. Irving submitted and moved adoption of the following nominees for the 1975–76 session. This motion was seconded by W. Sulzbacher and approved by the Board:

President-elect
Florence Forziati
Patricia Sarvella

Secretary
John Honig
Alfred Weissler

Treasurer
Richard Foote
Norman Griffiths

Managers-at-Large
Jean Boek
Howard Noyes
Charles Rader
Leland Whitelock

Membership Committee.—The membership committee submitted Alan S. Whelihan as nominee for fellowship; Shou Shan Fan, representing the American Society of Civil Engineers; Irving

H. Malitson representing the Optical Society of America; and Dick Duffey representing the American Nuclear Society. In Dr. Forziati's absence Dr. Irving made a motion that all be accepted as fellows. All four were accepted unanimously by the Board.

Symposium Committee.—Dr. Stern announced that Dr. A. Forziati was Chairman of the Symposium scheduled for March 13–14, 1975 on "Energy Recovery From Solid Wastes."

Journal.—Dr. Foote reported that the June issue of the Journal was out and that the cost was what had been anticipated.

Scientific Achievement Awards.—Dr. Kelso Morris reported that the Scientific Achievement Awards program was underway, and that he had been approached by a member of the Dept. of Psychology at George Mason University regarding the absence of an award in scientific achievement in Psychology. This was discussed by the Board. Since there was no delegate representing an affiliate society, the matter was sent to the Policy Planning Committee for consideration.

New Business.—Dr. Abraham made a motion, seconded by Mary Louise Robbins, that the Academy consider for adoption a plan to organize the affiliates into divisions as a possible solution to some of the Academy's problems. The tentative divisional structure consists of: 1) Physical Sciences and Math, 2) Biological and Medical Societies, 3) Engineering, 4) Historical and Geographical. The concern was to improve communications by forming divisions which could bring more societies together, thereby cutting down the number of individual meetings. It would also serve to broaden the disciplines by providing interdisciplinary programs through joint symposia, panel discussions, etc. Other thoughts were 1) that the membership of the Academy would increase since each division would have its own set of officers, and 2) that funding might be more easily obtained. Dr. Rupp made an amendment to the motion, namely that the name be changed from Biological and Medical Societies to Life Sciences, which should include the Historical and Geographical Societies. There was considerable discussion on the proposed divisional structure. It was decided that a letter should be prepared, outlining the divisional structure and the advantages of such a grouping, to go to the Presidents of affiliated societies with a copy to the delegate asking them to discuss the plan with their societies and report the reactions.

Dr. Robbins discussed the plans for a joint WAS-Affiliate program, Public Understanding of Science, and the possible use of educational television. She indicated that a statement had been drafted and will be sent with the letter on the divisional structure to Presidents of all affiliated societies with a copy to delegates.—Mary Aldridge, *Secretary*.

NEW FELLOWS

Joseph F. Coates, Office of Technology Assessment, U.S. Congress, in recognition of his original contributions to the application of scientific knowledge and thought through the technological assessment of programs designed to ameliorate problems of human existence. *Sponsors*: Jean K. Boek, Alfred Weissler, Mary Louise Robbins.

Anne R. Headley, Senior Professional Consultant, Federal Power Commission, in recognition of her scientific approach to solutions of problems of human existence, resulting from her study of and continuing intensive interest in the biological and social sciences. *Sponsors*: Jean K. Boek, Alphonse F. Forziati, Mary Louise Robbins.

Marian M. Schnepfe, Chemist, U.S. Geological Survey, in recognition of her work in analytical chemistry, especially applied to rocks and minerals. *Sponsors*: Charles R. Naeser, Reuben E. Wood, Theodore P. Perros.

ANNUAL REPORT OF THE TREASURER FOR 1974

Receipts and Income

Dues (members and fellows)	$12,748.75
Journal	
Subscriptions	4,124.00
Sale of Reprints (reimbursements from authors)	1,980.85
Sale of back issues	130.25
Sale of Symposium III issues & tapes	342.49
Investment Income (cash dividends & capital gains received in cash. Total does not include capital gains received in shares: WMI 14 shares)	2,932.25
Reimbursements	
Philosophical Society for Academy Services (for personnel, rent, telephone, print, mail & addressograph)	4,473.30
Geological Society for Academy Services (for personnel, rent, telephone, print, mail & addressograph	1,756.51
JBSEE for Academy services (for personnel, rent, telephone, print, mail & addressograph)	737.70
Board dinners (including awards & annual)	1,768.71
Grants-in-Aid (reimbursements for summer Science & Grants)	1,250.44
Miscellaneous	71.42
Contributions	150.00
Miscellaneous	70.00
Grants (EPA & DOT for Sym. III)	4,997.50
Total income	$37.534.17

Expenses and Disbursements

Journal	
Manufacturing cost	$ 7,524.07
Reprints (reimbursed by authors)	997.83
Honorarium to Editor	1,000.00
Symposium III general expenses	545.90
Office Expenses	
Rent (Jan. thru Dec.)	1,975.08
Telephone	328.08

Supplies	25.95
Office equipment	88.14
FASEB Misc.	204.11
Salary	10,057.32
FICA	588.12
Personnel benefits	606.16
Meetings	
Arrangements (includes print, mail, addressograph, xerox, Board, Committee & General Office)	2,996.07
Postoffice for mailing permits (WAS, Phil., Geological, & JBSEE)	450.00
Board dinners & Auditorium rental (dinners reimbursed)	2,774.83
Arrangements for Philosophical Society (reimbursed)	1,684.24
Arrangements for Geological Society (reimbursed)	780.29
JBSEE activities (reimbursed)	349.24
Encouragement of Science Talent (Jr. Academy)	$ 112.00
Grants-in-Aid (reimbursed by AAAS)	910.44
Contributions	
Summer Science at AU, reimbursed by AAAS	360.00
JBSEE	300.00
Loan (Payment to Dr. Forziati)	200.00
Miscellaneous	473.63
Total expenses and disbursements	$35,331.50

Capital Assets and Cash

The capital assets are in mutual funds the total market value of which on Dec. 30, 1974 was $43,810.85. The total market value for past years is as follows:

Dec. 31, 1969	$69,892.48	Dec. 31, 1971	$71,027.22	Dec. 31, 1973	$57,852.01
Dec. 31, 1970	85,311.54	Dec. 31, 1972	73,835.59		

The savings account at Perpetual Building Association plus interest is $755.76. Personal property, mostly in office equipment and furniture, is valued at an estimated $2000. The checking account balance on Dec. 31, 1974 was $7,313.41.

WASHINGTON JUNIOR ACADEMY OF SCIENCES

Checking Account	
Balance as of 12/31/73	$2,552.08
Receipts	219.00
Total	2,771.08
Disbursements	826.42
Balance as of 12/31/74	1,944.66

Savings Account	
Guaranteed Security Certificate	
Purchased 11/68	$2000.00
Interest to date	857.10
Total	$2857.10

Nelson W. Rupp, *Treasurer*

Raymond Davis

Raymond Davis passed away, in his sleep, in the early morning of September 5, 1974, at his home in Washington, DC. He was internationally known for his contributions to photographic sensitometry, colorimetry, and microphotography.

Mr. Davis joined the National Bureau of Standards as a photographer in 1911. His camera and his insatiable curiosity led him to all corners of the Bureau. By 1917, he was constructing equipment for the evaluation of photographic materials. He was an ingenious designer and often made his own apparatus. He insisted on "doing it right the first time" and when a job was done, he could stand back, puff his pipe, watch the thing work, and enjoy the fulfilling sense of satisfaction that only the true craftsman knows. His time-lapse motion picture camera, with automatic exposure control, photographed the construction of the Industrial Building at the National Bureau of Standards, in 1918.

By that time he had measured spectrosensitivities, the resolving powers, and several sensitometric characteristics of all available American negative materials. The Photographic Technology Section was established in 1920, with Davis as Chief. In the mid-20's, there was a clear need for a standard light source for sensitometry, so that laboratories could compare sensitometric evaluations on a uniform basis. Davis and K. S. Gibson proposed the use of the spectral distribution of daylight obtained by an incandescent source and a liquid filter. This proposal was adopted not only nationally, but internationally, for sensitometry. In 1931, Davis-Gibson filters were adopted by the International Commission on Illumination for use in photometry and colorimetry. The Davis-Gibson filters are used to this day in national and international standard light sources.

Davis introduced the concept of "correlated color temperature" to characterize light sources of nearly Planckian spectral distribution. This is an indispensable concept in modern photography, lighting practice, and colorimetry.

He made important contributions to every aspect of photographic sensitometry. His sensitometer was designed to provide continuous exposures or intermittent exposures of various kinds. He discovered that an intermittent exposure can sometimes have a greater photographic effect than a continuous exposure of equivalent energy, and that the effect might be positive in the toe of the characteristic curve and negative in the shoulder, or vice versa. His sensitometric processing machine was the model for such machines built for various government agencies, for many decades. For the sensitometric evaluation of photographic papers, he devised a method of finding the slope of the effective straight line portion of the characteristic curve, which he called "bar-gamma". This technique has been widely used in paper sensitometry and was incorporated in American National Standards. The historic research of Carroll and Hubbard, elucidating the process of emulsion making, was assisted by Davis's work in sensitometry.

Until 1932, The Federal Bureau of Investigation relied on NBS for scientific support and Davis was an important contributor to their investigations. He invented an ingenious technique for photographing the entire cylindrical surface of a bullet, as a continuous photograph on a single film. He invented a remarkably unusual application of the photographic process to the recovery of information from documents charred beyond legibility by fire. He assisted in the scientific analysis of evidence related to the kidnapping of Col. Charles A. Lindbergh's son.

In the early 30's, NBS did a lot of research on microfilm for archival purposes. Congress enacted a law permitting

the destruction of federal records on paper, if they had been copied on film meeting the standards of NBS. Davis played a role in that research and was responsible for implementing the law. Existing national and international standards for archival microfilm practice are largely based on that early work. In 1940, he designed a resolution chart for routine testing of microcopying systems. Although I made minor modifications in 1963, the basic pattern is still in use. It is specified in national and international standards and has been issued in larger numbers than any other resolution test chart in the world. Davis and Durand experimented with the relationship of legibility to resolving power and, on the basis of their results, I derived the quality index and legibility equation used throughout the microfilm industry today.

Early in World War II, Davis formulated a paint for marking military vehicles so that the markings would have minimum contrast in aerial photographs, but be legible on the ground. He devised photographic templates used in locating submarines, a technique for measuring dimensional changes of aerial films, and an instrument for recording aircraft engine temperatures.

To meet the demand on the short supply of handmade reticles and precisely graduated circles for navigational and fire control instruments, Chester Pope and Mr. Davis developed a new light-sensitive resist for producing such scales on glass by photoetching. No one dreamed at that time that these techniques were to be further developed and be widely used in the production of microminiature electronic components. In Davis's lifetime this technology paved the way to man's exploration of the moon, instrumental exploration of the planets, revolutionary advances in aircraft instrumentation, and the development of inexpensive miniature computers.

There was hardly a federal or military agency that did not call upon him for expert advice or assistance. He organized and served for many years as Chairman of the Federal Photographic and Photolithographic Specifications Committee.

He helped establish the standardization of photographic materials and processes in the American Standards Association, predecessor of the American National Standards Institute. From 1938 until his retirement in 1958, he was very active in nearly every facet of national photographic standardization and continued to participate after he retired, as a representative of the Optical Society of America. He represented the United States at the meetings of the Photography and Cinematography Committees of the International Organization for Standardization, in England, in 1958. He was a soft-spoken and modest man, but stood courageously for doing things right and argued persistently and persuasively for his position on standards.

He was a Charter Member and the first President of the Society of Photographic Engineers, founded on January 21, 1947. This society merged with the Technical Section of the Photographic Society of America in 1957, to become the Society of Photographic Scientists and Engineers. He was one of the first two Fellows of SPSE, along with John A. Maurer, having been awarded that honor in 1954. He was a Fellow of the Optical Society of America, Fellow of the Washington Academy of Sciences, Fellow of the American Physical Society, and member of the American Chemical Society, International Congress of Photography, Philosophical Society of Washington, Chemical Society of Washington, and one of the founders of the Federal Photographers. He was a Registered Engineer in the District of Columbia.

He thoroughly enjoyed his work, characterizing it as "playing around, while Uncle Sam provides the toys." That attitude toward long hours of hard work elevated him from photographer to internationally recognized scientist and engineer. His joyful interplay with nature made life more enjoyable for all who knew him. Happily, his contributions are

so widespread and lasting that we will often be reminded of him.

He leaves his wife, Dr. Marion Maclean Davis, a well recognized authority on the chemistry of acids and bases in inert solvents, who is retired from NBS but continues writing and editing in her field; two sons, Dr. Raymond Davis, Jr., who heads the Brookhaven Solar Neutrino Observatory, and Col. Warren P. Davis, U.S. Army, retired, Senior Research Scientist of the American Institutes for Research; eight grandchildren, all of whom are in or planning careers in science; and two great grandchildren.

—C. S. McCamy
Macbeth Color and Photometry Div.
Kollmorgen Corp.
Newburgh, N.Y.

S. M. Dohanian

(Addendum)

In acknowledging receipt of a reprint of my obituary of S. M. Dohanian (J. Wash. Acad. Sci. 64(3): 250–251) which I sent to the Commonwealth Institute of Entomology in London, the Librarian volunteered the information that they have found in their catalogs an additional 7 papers published by Dohanian. The references are as follows:

1915. Notes on the external anatomy of *Boreus brumalis* Fitch. Psyche *22*: 120–123.

1920. Mosquito control in a southern army camp. J. Econ. Entomol. *13*: 350–354.

1927. Preliminary experiments for the control of certain European vine-moths by fumigating with Cyanogas calcium cyanide. Psyche *34*: 146–156.

1927. Some of the important forest insects of western Europe. J. Econ. Entomol. *20*: 310–316.

1937. The search in the American tropics for beneficial insects for introduction into Puerto Rico. Agricultural Notes. Puerto Rico Exp. Sta. No. 76, 7 pp. (multigraphed).

1937. The importation of coccinellid enemies of diaspine scales into Puerto Rico. J. Agr. Univ. Puerto Rico *21*: 243–247.

1944. Control of the filbert worm and filbert weevil by orchard sanitation. J. Econ. Entomol. *37*: 764–766.

—Mortimer D. Leonard
Collaborator, Agr. Res. Serv., USDA
2480 16th St. N.W.
Washington, D.C. 20009

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Type manuscripts on white bond paper either 8½ by 11 or 8 by 10½ inches. Double space all lines, including those in abstracts, tables, legends, quoted matter, acknowledgments, and references cited. Number pages consecutively. Place your name and complete address in the upper right hand corner of the title page.

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Type on a separate sheet at the end of the manuscript. Make the abstract intelligible without reference to the text of the paper. Write an informative digest of the significant content and conclusions, not a mere description. Generally, the abstract should not exceed 3% of the text.

Footnotes

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Limit references within the text and in synonymies to author and year (and page if needed). In a "Reference Cited" section, list alphabetically by senior author only those papers you have included in the text. Likewise, be sure all the text references are listed. Type the "References Cited" section on a separate sheet after the last page of text. Abbreviations should follow the *USA Standard for Periodical Title Abbreviations*, Z39.5-1963.

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Directory Issue

CONTENTS

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This journal, the official organ of the Washington Academy of Sciences, publishes historical articles, critical reviews, and scholarly scientific articles; proceedings of meetings of the Academy and its Board of Managers; and other items of interest to Academy members. The *Journal* appears four times a year (March, June, September, and December) — the September issue contains a directory of the Academy membership.

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DELEGATES TO THE WASHINGTON ACADEMY OF SCIENCES, REPRESENTING THE LOCAL AFFILIATED SOCIETIES

Philosophical Society of Washington	Ralph P. Hudson
Anthropological Society of Washington	Jean K. Boek
Biological Society of Washington	Inactive
Chemical Society of Washington	Robert F. Cozzens
Entomological Society of Washington	Maynard Ramsay
National Geographic Society	Alexander Wetmore
Geological Society of Washington	Charles Milton
Medical Society of the District of Columbia	Inactive
Columbia Historical Society	Paul H. Oehser
Botanical Society of Washington	Conrad B. Link
Society of American Foresters	Alfred A. Weiner
Washington Society of Engineers	George Abraham
Institute of Electrical and Electronics Engineers	George Abraham
American Society of Mechanical Engineers	Michael Chi
Helminthological Society of Washington	James H. Turner
American Society for Microbiology	Thomas Cook
Society of American Military Engineers	H.P. Demuth
American Society of Civil Engineers	Shou Shan Fan
Society for Experimental Biology and Medicine	Donald Flick
American Society for Metals	Glen W. Wensch
International Association for Dental Research	Norman H.C. Griffiths
American Institute of Aeronautics and Astronautics	Franklin Ross
American Meteorological Society	A. James Wagner
Insecticide Society of Washington	Robert J. Argauer
Acoustical Society of America	Gerald J. Franz
American Nuclear Society	Dick Duffy
Institute of Food Technologists	William Sulzbacher
American Ceramic Society	Inactive
Electrochemical Society	Delegate not appointed
Washington History of Science Club	Inactive
American Association of Physics Teachers	Bernard B. Watson
Optical Society of America	Ronald W. Weynant
American Society of Plant Physiologists	Walter Shropshire
Washington Operations Research Council	John G. Honig
Instrument Society of America	Inactive
American Institute of Mining, Metallurgical and Petroleum Engineers	Carl H. Cotterill
National Capitol Astronomers	John A. Eisele
Mathematical Association of America	Patrick Hayes
D.C. Institute of Chemists	Miloslav Recheigl, Jr.
D.C. Psychological Association	John O'Hare

Delegates continue in office until new selections are made by the respective societies.

THE DIRECTORY OF THE ACADEMY FOR 1975

Foreword

The present, 50th issue of the Academy's directory is again this year issued as part of the June number of the Journal. As in previous years, the alphabetical listing is based on a postcard questionnaire sent to the Academy membership. Members were asked to update the data concerning address and membership in affiliated societies by June 30, 1975. In cases in which cards were not received by that date, the address appears as it was used during 1974, and the remaining data were taken from the directory for 1974. Corrections should be called to the attention of the Academy office.

Code for Affiliated Societies, and Society Officers

1 The Philosophical Society of Washington (1898)

President:	Ralph P. Hudson, NBS, Washington, D.C. 20234
Vice-President:	Robert J. Rubin, 3308 McKinley St., N.W., Washington, D.C. 20015
Secretary:	Patricia S. Willis, 2824 W. George Mason Rd., Falls Church, Va. 22042
Delegate:	Ralph P. Hudson

2 Anthropological Society of Washington (1898)

President:	Philleo Nash, Dept. of Anthropology, American Univ., Washington, D.C. 20016
President-elect:	Dr. Robert Humphrey, George Washington University, Washington, D.C.
Secretary:	Marjorie G. Whiting, 407 5th St., S.E., Washington, D.C. 20003
Delegate:	Jean K. Boek, Dir., Div. of Special Studies, National Graduate Univ., 3408 Wisconsin Ave., N.W., Washington, D.C. 20016

3 Biological Society of Washington (1898)

President:	Joseph Rosewater, Smithsonian Institution, Washington, D.C. 20560
Secretary:	Richard C. Banks, Smithsonian Institution, Washington, D.C. 20560

4 Chemical Society of Washington (1898)

President:	Robert F. Cozzens, George Mason Univ., Fairfax, Va. 22030
President-elect:	David Venezky, Naval Res. Lab., Washington, D.C. 20375
Secretary:	John Moody, NBS, Chemistry, Bldg. 222, Washington, D.C. 20234
Delegate:	Robert F. Cozzens

5 Entomological Society of Washington (1898)

President:	H. Ivan Rainwater, Rm. 635, Federal Bldg., Hyattsville, Md. 20782
President-elect:	George C. Steyskal, W-617, NMNH, Washington, D.C. 20560
Secretary:	Theodore J. Spilman, W-605, NMNH, Washington, D.C. 20560
Delegate:	Maynard J. Ramsay, Rm. 660, Federal Bldg., Hyattsville, Md. 20782

6 National Geographic Society (1898)

President:	Melvin M. Payne, 17th & M Sts., N.W., Washington, D.C. 20036
Vice-President & Secretary:	Robert E. Doyle, 17th & M Sts., N.W., Washington, D.C. 20036
Delegate:	Alexander Wetmore, Smithsonian Institution, Washington, D.C. 20560

7 Geological Society of Washington (1898)

President:	Joshua I. Tracey, Jr., U.S. Geological Survey, Reston, Va. 22092
Vice-President:	Dallas L. Peck, U.S. Geological Survey, Reston, Va. 22092
Secretary:	Penelope M. Hanshaw, U.S. Geological Survey, Reston, Va. 22092
Delegate:	Charles Milton, Dept. of Geology, George Washington Univ., Washington, D.C. 20005

8 Medical Society of the District of Columbia (1898)

President:	William S. McCune
President-elect:	Frank S. Bacon
Secretary:	Thomas Sadler
Delegate:	Not appointed

9 Columbia Historical Society (1899)

President: Hemer T. Rosenberger, 1307 New Hampshire Ave., N.W., Washington, D.C. 20036
Vice-President: Wilcomb E. Washburn, Smithsonian Institution, Washington, D.C. 20560
Secretary: Edward F. Gerber, 1233 30th St., N.W., Washington, D.C. 20007
Delegate: Paul H. Oehser, National Geographic Society, Washington, D.C. 20036

10 Botanical Society of Washington (1902)

President: Robert W. Read, Dept. of Botany, Smithsonian Inst., Washington, D.C. 20560
Vice-President: Beryl Simpson, Dept. of Botany, Smithsonian Inst., Washington, D.C. 20560
Secretary: Dr. Joseph Higgins, Plant Variety Protection Office, USDA, Center Bldg., 6525 Belcrest Rd., Hyattsville, Md. 20782
Delegate: Conrad B. Link, Dept. of Horticulture, Univ. of Md., College Park, Md. 20742

11 Society of American Foresters, Washington Section (1904)

Chairman: Arthur V. Smyth, 1625 Eye St., N.W., Washington, D.C.
Secretary: George Cheek, 1619 Mass. Ave., N.W., Washington, D.C.
Delegate: Alfred A. Weiner, 3202 South Agriculture Bldg., Washington, D.C. 20250

12 Washington Society of Engineers (1907)

President: George Abraham, 3107 Westover Dr., S.E., Washington, D.C. 20020
Vice-President: Joseph H. Seelinger, 5367 28th St., N.W., Washington, D.C. 20015
Secretary: John W. Lanier, 12902 Sturbridge Rd., Weedbridge, Va. 22191
Delegate: George Abraham

13 Institute of Electrical & Electronics Engineers, Washington Section (1912)

Chairman: Alvin Reiner, 11243 Bybee St., Silver Spring, Md. 20902
Vice-Chairman: J. J. Kelleher, 3717 King Arthur Rd., Annandale, Va. 22032
Secretary: Herst Gerlach, 4521 Cheltenham Dr., Bethesda, Md. 20014
Delegate: George Abraham, 3107 Westover Dr., S.E., Washington, D.C. 20020

14 American Society of Mechanical Engineers, Washington Section (1923)

Chairman: William Walston, Dept. of Mechanical Engineering, Univ. of Md., College Park, Md. 20742
Vice-Chairman: D. Howard, Postal Service I & D Inst., 7900 Wisconsin Ave., N.W. Bethesda, Md. 20014
Secretary: Charles Miller, 12013 Hamden Ct., Oakton, Va. 22124
Delegate: Michael Chi, Dept. of Civil Eng. Catholic Univ., Washington, D.C. 20064

15 Helminthological Society of Washington (1923)

President: Robert S. Isenstein, Animal Parasitology Inst., BARC-East, Beltsville, Md. 20705
Vice-President: A. Morgan Golden, Nematology Lab., Plant Protection Inst., BARC-West, Beltsville, Md. 20705
Secretary: William R. Nickle, Nematology Lab., Plant Protection Inst., BARC-West, Beltsville, Md. 20705
Delegate: James H. Turner, Division of Res. Grants, NIH, Westwood Bldg., Rm. A25, Bethesda, Md. 20014

16 American Society for Microbiology, Washington Branch (1923)

President: Joseph C. Olson, Jr., Food & Drug Adm., Washington, D.C.
Vice-President: Charles H. Zierdt, NIH, Bethesda, Md. 20014
Secretary: June W. Bradlaw, Food & Drug Adm., Washington, D.C.
Delegate: Thomas Cook, Dept. of Microbiology, Univ. of Md., College Park, Md. 20742

17 Society of American Military Engineers, Washington Post (1927)

President: Brig. Gen. William Wray, USA, Office Chief of Engineers, Forrestal Bldg., Washington, D.C. 20314
Vice-President: Cdr. Theodore J. Wojnar, USCG, Coast Guard Hdqtrs., Attn: (ECV), Washington, D.C. 20590

Secretary:	Lt. Col. Ancil R. Pressley, USA, Office Chief of Engineers, Forrestal Bldg., Washington, D.C. 20314
Delegate:	Cdr. Hal P. Demuth, 4025 Pinebrook Rd., Alexandria, Va. 22310

18 American Society of Civil Engineers, National Capital Section (1942)

President:	Homer D. Willis, Engineering Div., Office of Chief Engineer, Corps of Engineers, Washington, D.C. 20314
Vice-President:	L. G. Byrd, Wilbur Smith & Assoc., 2921 Telestar Ctr., Falls Church, Va. 22042
Secretary:	Robert D. Wolff, Planning Div., Office of Chief Engineers, Corps of Engineers, Washington, D.C. 20314
Delegate:	Shou-shan Fan, 2313 Glenallan Ave., #202, Silver Spring, Md. 20906

19 Society for Experimental Biology & Medicine, D.C. Section (1952)

President:	Benjamin H. Bruckner, Natl. Inst. for Occupational Safety & Health, Rm. 3-44, Park Bldg., 5600 Fishers Lane, Rockville, Md. 20852
President-elect:	Leon Prosky, 9521 Cherry Oak Ct., Burke, Va. 22015
Secretary:	Juan Penhos, 5402 Surrey St., Chevy Chase, Md. 20015
Delegate:	Donald Flick, 930 S. 19th St., Arlington, Va. 22015

20 American Society for Metals, Washington Chapter (1953)

Chairman:	Klaus M. Zwilsky, U.S. Atomic Energy Comm., Washington, D.C. 20545
Vice-chairman:	Alan H. Rosenstein, Air Force Office of Scientific Res., 1400 Wilson Blvd., Arlington, Va. 22209
Secretary:	Joseph Malz, NASA, Code RWM, Washington, D.C. 20546
Delegate:	Glen W. Wensch, U.S. Atomic Energy Comm., Washington, D.C. 20545

21 International Association for Dental Research, Washington Section (1953)

President:	Robert W. Longton, Dental Sciences Dept., Naval Med. Res. Inst., NNMC, Bethesda, Md. 20014
Vice-President:	Nelson W. Rupp, Dental Res., NBS, Washington, D.C. 20234
Secretary:	Donald W. Turner, Dental Sciences Dept., Naval Med. Res. Inst., NNMC, Bethesda, Md. 20014
Delegate:	Norman H. C. Griffiths, 3100 20th St., N.E., Washington, D.C. 20018

22 American Institute of Aeronautics and Astronautics, National Capital Section (1953)

President:	Philip R. Compton, 6303 Mori St., McLean, Va. 22101
Vice-President:	Jack Suddreth, Code RLC/Aero. Prop. Div., NASA Headquarters, Washington, D.C. 20546
Secretary:	Paul M. Burris, The Boeing Co., 955 L'Enfant Plaza North, S.W., Washington, D.C. 20024
Delegate:	Franklin J. Ross, Deputy for Rqmts., Off. Asst. Sec. of A.F., The Pentagon, Rm. 4E973, Washington, D.C. 20330

23 American Meteorological Society, D.C. Chapter (1954)

Chairman:	John S. Perry, National Academy of Science, Rm. JH426C, 2101 Constitution Ave., Washington, D.C. 20418
Vice-Chairman:	James L. Rasmussen, US Gate Project Office, NOAA/EM6, 6010 Executive Blvd., Rockville, Md. 20852
Secretary:	H. Michael Mogil, NOAA/NWS/EWB/W117X1, 1426 Gramax Bldg., 8060 13th St., Silver Spring, Md. 20910
Delegate:	A. James Wagner, NOAA/MWS/NMC Wel, 604 World Weather Bldg., 5200 Auth Rd., Washington, D.C. 20233

24 Insecticide Society of Washington (1959)

President:	Richard Back, Union Carbide, 1730 Pa. Ave., N.E., Suite 1250, Washington, D.C. 20006
President-elect:	John W. Kennedy, APHIS, USDA, Hyattsville, Md.
Secretary:	John Neal, ARS, ARC, Bldg. 467, Beltsville, Md. 20705
Delegate:	Robert Argauer, ARS, ARC, Bldg. 309, Beltsville, Md. 20705

25 Acoustical Society of America (1959)

Chairman:	John A. Molino, Sound Section, NBS, Washington, D.C. 20234
Vice-chairman:	Charles T. Molloy, 2400 Claremont Dr., Falls Church, Va. 22043
Secretary:	William K. Blake, Naval Ship R&D Ctr., Bethesda, Md. 20034
Delegate:	Gerald J. Franz, 9638 Culver St., Kensington, Md. 20795

26 American Nuclear Society, Washington Section (1960)

President:	Parks Honeywell, NUS Corp., Rockville, Md. 20850
Vice-President:	Andre Gage, Potomac Electric Power, 1900 Pa. Ave., Washington, D.C.
Secretary:	Peter F. Wiggins, US Naval Academy, Annapolis, Md.
Delegate:	Dick Duffey, Nuclear Engineering, Univ. of Md., College Park, Md. 20742

27 Institute of Food Technologists, Washington Section (1961)

Chairman:	Tannous Khalil, Giant Foods, Inc., Landover, Md. 20785
Vice-chairman:	Florian C. Majorack, Food & Drug Adm., Washington, D.C. 20204
Secretary:	Glenn V. Brauner, National Canners Assoc., Washington, D.C. 20036
Delegate:	William Sulzbacher, 8527 Clarkson Dr., Fulton, Md. 20759

28 American Ceramic Society, Baltimore-Washington Section (1962)

Chairman:	W. T. Bakker, General Refractories Co., P.O. Box 1673, Baltimore, Md. 21203
Chairman-elect:	L. Biller, Glidden-Dirkee Div., SCM Corp., 3901 Hawkins Point Rd., Baltimore, Md. 21226
Secretary:	Edwin E. Childs, J. E. Baker Co., 232 E. Market St., York, Pa. 17405
Delegate:	None appointed

29 Electrochemical Society, National Capital Section (1963)

Chairman:	Judith Ambrus, Naval Surface Weapons Ctr., White Oak, Md. 20910
Vice-chairman:	John B. O'Sullivan, 7724 Glenister Dr., Springfield, Va. 22152
Secretary:	John Ambrose, NBS, Washington, D.C. 20234
Delegate:	None appointed

30 Washington History of Science Club (1965)

Chairman:	Richard G. Hewlett, Atomic Energy Comm.
Vice-chairman:	Deborah Warner, Smithsonian Institution
Secretary:	Dean C. Allard
Delegate:	None appointed

31 American Association of Physics Teachers, Chesapeake Section (1965)

President:	William Logan, D.C. Teachers College, 2565 Georgia Ave., Washington, D.C. 20001
Vice-President:	Eugenie V. Mielczarek, George Mason Univ., 4400 University Dr., Fairfax, Va. 22030
Secretary:	John B. Newman, Towson State College, Towson, Md. 21204
Delegate:	Bernard B. Watson, 6108 London Lane, Bethesda, Md. 20034

32 Optical Society of America, National Capital Section (1966)

President:	Ronald W. Waynant, 13101 Claxton Dr., Laurel, Md. 20811
Vice-President:	James Heaney, NASA/GODDARD, Code 722, Greenbelt, Md. 20771
Secretary:	Marilyn Dodge, NBS, A251, Physics, Washington, D.C. 20234
Delegate:	Ronald W. Waynant

33 American Society of Plant Physiologists, Washington Section (1966)

President:	Bert Drake, RBL, 12441 Parklawn Dr., Rockville, Md. 20852
Vice-President:	Aref Abdul-baki, USDA, Post Harvest Physiology Lab., Beltsville, Md. 20705
Secretary:	Dale Blevins, Dept. of Botany, Univ. of Md., College Park, Md. 20742
Delegate:	W. Shropshire, Jr., RBL, 12441 Parklawn Dr., Rockville, Md. 20852

34 Washington Operations Research Council (1966)

President:	Frank T. Trippi, 5809 Clermont Dr., Alexandria, Va. 22310
President-elect:	Craig C. Sherbrooke, 8513 Kingsgate Rd., Potomac, Md. 20854
Secretary:	Neal D. Glassman, 1 Paddock Ct., Potomac, Md. 20854
Delegate:	John G. Honig

35 Instrument Society of America, Washington Section (1967)

President:	Francis C. Quinn
President-elect:	John I. Peterson
Secretary:	Frank L. Carou
Delegate:	None appointed

36 American Institute of Mining, Metallurgical & Petroleum Engineers (1968)

Chairman:	Carl H. Cotterill, US Bureau of Mines, 2401 E St., N.W., Washington, D.C. 20241
Vice-chairman:	Herbert R. Babitzke, US Bureau of Mines, 2401 E St., N.W., Washington, D.C. 20241
Secretary:	John R. Babey, US Dept. of Interior, 18th & F St., N.W., Washington, D.C. 20240
Delegate:	Carl H. Cotterill

37 National Capital Astronomers (1969)

President:	John A. Eisele, 3310 Curtis Dr., No. 202, Hillcrest Heights, Md. 20023
Vice-President:	Henning E. Leidecker, 4811 Avondale Rd., Washington, D.C. 20018
Secretary:	Estelle Finkle, 939 26th St., N.W., Washington, D.C. 20037
Delegate:	John A. Eisele, 3310 Curtis Dr., #202, Hillcrest Heights, Md. 20023

38 Maryland-District of Columbia and Virginia Section of Mathematical Assoc. of America (1971)

Chairman:	Geraldine A. Coon, Goucher College, Baltimore, Md.
Secretary:	John Smith, George Mason University, Fairfax, Va.
Delegate:	Patrick Hayes, Analytic Services Inc., 5613 Leesburg Pike, Falls Church, Va. 22041

39 D.C. Institute of Chemists (1973)

President:	Kelso B. Morris, 1448 Leegate Rd., N.W., Washington, D.C. 20012
President-elect:	Leo Schubert, 8521 Beech Tree Rd., Bethesda, Md. 20034
Secretary:	Fred D. Ordway, 2816 Fall Jax Dr., Falls Church, Va. 22042
Delegate:	Miloslav Rechcigl, Jr., 1703 Mark Lane, Rockville, Md. 20852

40 The D.C. Psychological Association (1975)

President:	Margaret Ives, 302 Rucker Place, Alexandria, Va. 22301
President-elect:	Lee Gurel, 2723 Woodley Place, N.W., Washington, D.C. 20003
Secretary:	John F. Borriello, 4620 North Park Ave., Chevy Chase, Md. 20015
Delegate:	John J. O'Hare, 301 G St., S.W., #824, Washington, D.C. 20024

M = Member; F = Fellow; E = Emeritus member. Numbers in parentheses refer to numerical code in foregoing list of affiliated societies.

A

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BERLINER, ROBERT W., M.D., Dean, Yale School of Medicine, New Haven, Conn. 06510 (F)
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BYLAWS

Washington Academy of Sciences

Last Revised in February 1972

Article I. OBJECTIVES

Section 1. The purposes of the Washington Academy of Sciences shall be: (a) to stimulate interest in the sciences, both pure and applied, and (b) to promote their advancement and the development of their philosophical aspects by the Academy membership and through cooperative action by the affiliated societies.

Section 2. These objectives may be attained by, but are not limited to:

(a) Publication of a periodical and of occasional scientific monographs and such other publications as may be deemed desirable.
(b) Public lectures of broad scope and interest in the fields of science.
(c) Sponsoring a Washington Junior Academy of Sciences.
(d) Promoting science education and a professional interest in science among people of high school and college age.
(e) Accepting or making grants of funds to aid special research projects.
(f) Symposia, both formal and small informal, on any aspects of science.
(g) Scientific conferences.
(h) Organization of, or assistance in, scientific expeditions.
(i) Cooperation with other Academies and scientific organizations.
(j) Awards of prizes and citations for special merit in science.
(k) Maintaining an office and staff to aid in carrying out the purposes of the Academy.

Article II. MEMBERSHIP

Section 1. The membership shall consist of three general classes: members, fellows and patrons.

Section 2. Members shall be persons who are interested in and will support the objectives of the Academy and who are otherwise acceptable to at least two-thirds of the Committee on Membership. A letter or application form requesting membership and signed by the applicant may suffice for action by the Committee; approval by the Committee constitutes election to membership.

Section 3. Fellows shall be persons who by reason of original research or other outstanding service to the sciences, mathematics, or engineering are deemed worthy of the honor of election to Academy fellowship.

Section 4. Nominations of fellows shall be presented to the Committee on Membership as a form approved by the Committee. The form shall be signed by the sponsor, a fellow who has knowledge of the nominee's field, and shall be endorsed by at least one other fellow. An explanatory letter from the sponsor and a bibliography of the nominee's publications shall accompany the completed nomination form.

Section 5. Election to fellowship shall be by vote of the Board of Managers upon recommendation of the Committee on Membership. Final action on nominations shall be deferred at least one week after presentation to the Board, and two-thirds of the vote cast shall be necessary to elect.

Section 6. Each individual (not already a fellow) who has been nominated as a Delegate by a local affiliated society or who has been chosen to be the recipient of an Academy Award for Scientific Achievement shall be considered nominated for immediate election to fellowship by the Board of Managers without the necessity for compliance with the provisions of Sections 4 and 5.

Section 7. An individual of unquestioned eminence may be recommended by vote of the Committee on Membership Promotion for immediate election to fellowship by the Board of Managers, without the necessity for compliance with the provisions of Sections 4 and 5.

Section 8. Persons who have given to the Academy not less than one thousand (1,000) dollars or its equivalent in property shall be eligible for election by the Board of Managers as patrons (for life) of the Academy.

Section 9. Life members or fellows shall be those individuals who have made a single payment in accordance with Article III, Section 2, in lieu of annual dues.

Section 10. Members or fellows in good standing who are retired and are no longer engaged in regular gainful employment may be placed in emeritus status. Upon request to the treasurer for transfer to this status, they shall be relieved of the further payment of dues, beginning with the following January first; shall receive notices of meetings without charge; and at their request, shall be entitled to receive the Academy periodical at cost.

Section 11. Members or fellows living more than 50 miles from the White House, Washington, D.C., shall be classed as nonresident members or fellows.

Section 12. An election to any dues-paying class of membership shall be void if the candidate does not within three months thereafter pay his dues or satisfactorily explain his failure to do so.

Section 13. Former members or fellows who resigned in good standing may be reinstated upon application to the Secretary and approval by the Board of Managers. No reconsideration of the applicant's qualifications need be made by the Membership Committee in these cases.

Article III. DUES

Section 1. The annual dues of each class of members shall be fixed by the Board of Managers. No dues shall be paid by emeritus members and fellows, life members and fellows, and patrons.

Section 2. Members and fellows in good standing may be relieved of further payment of dues by making a single payment to provide an annuity equal to their annual dues. (See Article II, Section 9.) The amount of the single payment shall be computed on the basis of an interest rate to be determined by the Board of Managers.

Section 3. Members or fellows whose dues are in arrears for one year shall not be entitled to receive Academy publications.

Section 4. Members or fellows whose dues are in arrears for more than two years shall be dropped from the rolls of the Academy, upon notice to the Board of Managers, unless the Board shall otherwise direct. Persons who have been dropped from membership for nonpayment of dues may be reinstated upon approval of the Board and upon payment of back dues for two years together with dues for the year of reinstatement.

Article IV. OFFICERS

Section 1. The officers of the Academy shall be a President, a President-elect, a Secretary, and a Treasurer. All shall be chosen from resident fellows of the Academy.

Section 2. The President shall appoint all committees and such non-elective officers as are needed unless otherwise directed by the Board of Managers or provided in the Bylaws. He (or his substitute—the President-elect, the Secretary, or the Treasurer, in that order), shall preside at all meetings of the Academy and of the Board of Managers.

Section 3. The Secretary shall act as secretary to the Board of Managers and to the Academy at large. He shall conduct all correspondence relating thereto, except as otherwise provided, and shall be the custodian of the corporate seal of the Academy. He shall arrange for the publication in the Academy periodical of the names and professional connections of new members, and also of such proceedings of the Academy, including meetings of the Board of Managers, as may appropriately be of interest to the membership. He shall be responsible for keeping a register of the membership, showing such information as qualifications, elections, acceptances, changes of residence, lapses of membership, resignations and deaths, and for informing the Treasurer of changes affecting the status of members. He shall act as secretary to the Nominating Committee (see Art. VI, Sect. 2).

Section 4. The Treasurer shall be responsible for keeping an accurate account of all receipts and disbursements, shall select a suitable depository for current funds which shall be approved by the Executive Committee, and shall invest the permanent funds of the Academy as directed by that Committee. He shall prepare a budget at the beginning of each year which shall be reviewed by the Executive Committee for presentation to and acceptance by the Board of Managers. He shall notify the Secretary of the date when each new member qualifies by payment of dues. He shall act as business advisor to the Editor and shall keep necessary records pertaining to the subscription list. In view of his position as Treasurer, however, he shall not be required to sign contracts. He shall pay no bill until it has been approved in writing by the chairman of the committee or other persons authorized to incur it. The fiscal year of the Academy shall be the same as the calendar year.

Section 5. The President and the Treasurer, as directed by the Board of Managers, shall jointly assign securities belonging to the Academy and indorse financial and legal papers necessary for the uses of the Academy, except those relating to current expenditures authorized by the Board. In case of disability or absence of the President or Treasurer, the Board of Managers may designate the President-elect or a qualified Delegate as Acting President or an officer of the Academy as Acting Treasurer, who shall perform the duties of these officers during such disability or absence.

Section 6. An Editor shall be in charge of all activities connected with the Academy's publications. He shall be nominated by the Executive Committee and appointed by the President for an indefinite term subject to annual review by the Board of Managers. The Editor shall serve as a member of the Board.

Section 7. An Archivist may be appointed by the President. If appointed, he shall maintain the permanent records of the Academy, including important records which are no longer in current use by the Secretary, Treasurer, or other officer, and such other documents and material as the Board of Managers may direct.

Section 8. All officers and chairmen of standing committees shall submit annual reports at the May meeting of the Board of Managers.

Section 9. The Nominating Committee (Article IV, Section 2) shall prepare a slate listing two or more persons for each of the offices of President-elect, of Secretary and of Treasurer, and four or more persons for the two Managers-at-large whose terms expire each year and at least two persons to fill each vacant unexpired term of manager-at-large. The slate shall be presented for approval to the Board of Managers at its first meeting in October. Not later than November 15, the Secretary shall forward to each Academy Member and Fellow an announcement of the election, the committee's nomination for the offices to be filled, and a list of incumbents. Additional candidates for such offices may be proposed by any Member or Fellow in good standing by letter received by the Secretary not later than Dec. 1. The name of any eligible candidate so proposed by ten Members or Fellows shall be entered on the ballot.

Section 10. Not later than December 15, the Secretary shall prepare and mail ballots to members and fellows. Independent nominations shall be included on the ballot, and the names of the nominees shall be arranged in alphabetical order. When more than two candidates are nominated for the same office the voting shall be by preferential ballot in the manner prescribed by the Board of Managers. The ballot shall contain also a notice to the effect that votes not received by the Secretary before the first Thursday of January, and votes of individuals whose dues are in arrears for one year or more, will not be counted. The Committee of Tellers shall count the votes and report the results at the annual meeting of the Academy.

Section 11. The newly elected officers shall take office at the close of the annual meeting, the President-elect of the previous year automatically becoming President.

Article V. BOARD OF MANAGERS

Section 1. The activities of the Academy shall be guided by the Board of Managers, consisting of the President, the President-elect, the immediate past President, one Delegate from each of the affiliated societies, the Secretary, the Treasurer, six elected Managers-at-Large, and the Editor. The elected officers of the Academy shall hold like offices on the Board of Managers.

Section 2. One Delegate shall be selected by each affiliated society. He shall serve until replaced by his society. Each Delegate is expected to participate in the meetings of the Board of Managers and vote on behalf of his society.

Section 3. The Board of Managers shall transact all business of the Academy not otherwise provided for. A quorum of the Board shall be nine of its members.

Section 4. The Board of Managers may provide for such standing and special committees as it deems necessary.

Section 5. The Board shall have power to fill vacancies in its own membership until the next annual election. This does not apply to the offices of President and Treasurer (see Art. IV, Sect. 5), nor to Delegates (see Art. V, Sect. 2).

Article VI. COMMITTEES

Section 1. An Executive Committee shall have general supervision of Academy finances, approve the selection of a depository for the current funds, and direct the investment of the permanent

funds. At the beginning of the year it shall present to the Board of Managers an itemized statement of receipts and expenditures of the preceding year and a budget based on the estimated receipts and disbursements of the coming year, with such recommendations as may seem desirable. It shall be charged with the duty of considering all activities of the Academy which may tend to maintain and promote relations with the affiliated societies, and with any other business which may be assigned to it by the Board. The Executive Committee shall consist of the President, the President-elect, the Secretary and the Treasurer (or Acting Treasurer) ex officio, as well as two members appointed annually by the President from the membership of the Board.

Section 2. The President, with the approval of the Board of Managers, shall appoint a Nominating Committee of six Fellows of the Academy, at least one of whom shall be a past President of the Academy, and at least three of whom shall have served as Delegates for at least one year. The Chairman shall be a past President. (See Article IV, Section 9.)

Section 3. The President shall appoint in advance of the annual meeting an Auditing Committee consisting of three persons, none of whom is an officer, to audit the accounts of the Treasurer (Art. VII, Sect. 1).

Section 4. On or before the last Thursday of each year the President shall appoint a committee of three Tellers whose duty it shall be to canvass the ballots (Art. IV, Sect. 10, Art. VII, Sect. 1).

Section 5. The President shall appoint from the Academy membership such committees as are authorized by the Board of Managers and such special committees as necessary to carry out his functions. Committee appointments shall be staggered as to term whenever it is determined by the Board to be in the interest of continuity of committee affairs.

Article VII. MEETINGS

Section 1. The annual meeting shall be held each year in May. It shall be held on the third Thursday of the month unless otherwise directed by the Board of Managers. At this meeting the reports of the Secretary, Treasurer, Auditing Committee (see Article VI, Sect. 3), and Committee of Tellers shall be presented.

Section 2. Other meetings may be held at such time and place as the Board of Managers may determine.

Section 3. The rules contained in "Robert's Rules of Order Revised" shall govern the Academy in all cases to which they are applicable, and in which they are not inconsistent with the bylaws or special rules of order of the Academy.

Article VIII. COOPERATION

Section 1. The term "affiliated societies" in their order of seniority (see Art. VI, Sect. 2) shall be held to cover the:

Philosophical Society of Washington
Anthropological Society of Washington
Biological Society of Washington
Chemical Society of Washington
Entomological Society of Washington
National Geographic Society
Geological Society of Washington
Medical Society of the District of Columbia
Columbia Historical Society
Botanical Society of Washington
Washington Section of Society of American Foresters
Washington Society of Engineers
Washington Section of Institute of Electrical and Electronics Engineers
Washington Section of American Society of Mechanical Engineers
Helminthological Society of Washington
Washington Branch of American Society for Microbiology
Washington Post of Society of American Military Engineers
National Capital Section of American Society of Civil Engineers
District of Columbia Section of Society for Experimental Biology and Medicine
Washington Chapter of American Society for Metals
Washington Section of the International Association for Dental Research
Washington Section of American Institute of Aeronautics and Astronautics

D.C. Branch of American Meteorological Society
Insecticide Society of Washington
Washington Chapter of the Acoustical Society of America
Washington Section of the American Nuclear Society
Washington Section of Institute of Food Technologists
Baltimore-Washington Section of the American Ceramic Society
Washington-Baltimore Section of the Electrochemical Society
Washington History of Science Club
Chesapeake Section of American Association of Physics Teachers
National Capital Section of Optical Society of America
Washington Section of American Society of Plant Physiologists
Washington Operations Research Council
Washington Section of Instrument Society of America
American Institute of Mining, Metallurgical, and Petroleum Engineers
National Capital Astronomers
Maryland-District of Columbia-Virginia Section of the Mathematical Association of America
District of Columbia Institute of Chemists

and such others as may be hereafter recommended by the Board and elected by two-thirds of the members of the Academy voting, the vote being taken by correspondence. A society may be released from affiliation on recommendation of the Board of Managers, and the concurrence of two-thirds of the members of the Academy voting.

Section 2. The Academy may assist the affiliated scientific societies of Washington in any matter of common interest, as in joint meetings, or in the publication of a joint directory: Provided, it shall not have power to incur for or in the name of one or more of these societies any expense or liability not previously authorized by said society or societies, nor shall it without action of the Board of Managers be responsible for any expenses incurred by one or more of the affiliated societies.

Section 3. No affiliated society shall be committed by the Academy to any action in conflict with the charter, constitution, or bylaws of said society, or of its parent society.

Section 4. The Academy may establish and assist a Washington Junior Academy of Sciences for the encouragement of interest in science among students in the Washington area of high school and college age.

Article IX. AWARDS AND GRANTS-IN-AID

Section 1. The Academy may award medals and prizes, or otherwise express its recognition and commendation of scientific work of high merit and distinction in the Washington area. Such recognition shall be given only on approval by the Board of Managers of a recommendation by a committee on awards for scientific achievement.

Section 2. The Academy may receive or make grants to aid scientific research in the Washington area. Grants shall be received or made only on approval by the Board of Managers of a recommendation by a committee on grants-in-aid for scientific research.

Article X. AMENDMENTS

Section 1. Amendments to these bylaws shall be proposed by the Board of Managers and submitted to the members of the Academy in the form of a mail ballot accompanied by a statement of the reasons for the proposed amendment. A two-thirds majority of those members voting is required for adoption. At least two weeks shall be allowed for the ballots to be returned.

Section 2. Any affiliated society or any group of ten or more members may propose an amendment to the Board of Managers in writing. The action of the Board in accepting or rejecting this proposal to amend the bylaws shall be by a vote on roll call, and the complete roll call shall be entered in the minutes of the meeting.

ACT OF INCORPORATION OF THE WASHINGTON ACADEMY OF SCIENCES

We, the undersigned, persons of full age and citizens of the United States, and a majority being citizens of the District of Columbia, pursuant to and in conformity with sections 545 to 552, inclusive, of the Revised Statutes of the United States relating to the District of Columbia, as amended by an Act of Congress entitled "An Act to amend the Revised Statutes of the United States relating to

the District of Columbia and for other purposes," approved April 23, 1884, hereby associate ourselves together as a society or body corporate and certify in writing:

1. That the name of the society is the Washington Academy of Sciences.

2. That the term for which the Corporation is organized shall be perpetual.

3. That the Corporation is organized and shall be operated exclusively for charitable, educational and scientific purposes and in furtherance of these purposes and for no other purpose shall have, but not be limited to, the following specific powers and purposes:

   a. To encourage in the broadest and most liberal manner the advancement and promotion of science.
   b. To acquire, hold, and convey real estate and other property and to establish general and special funds.
   c. To hold meetings.
   d. To publish and distribute documents.
   e. To conduct lectures.
   f. To conduct, endow, or assist investigation in any department of science.
   g. To acquire and maintain a library.
   h. And, in general, to transact any business pertinent to an academy of sciences.

   Provided, however, that notwithstanding the foregoing enumerated powers, the Corporation shall not engage in activities, other than as an insubstantial part thereof, which are not in themselves in furtherance of its charitable, educational and scientific purposes.

4. That the affairs, funds, and property of the Corporation shall be in general charge of a Board of Managers, the number of whose members for the first year shall be nineteen, all of whom shall be chosen from among the members of the Academy.

5. That in the event of dissolution or termination of the Corporation, title to and possession of all the property of the Corporation shall pass to such organization, or organizations, as may be designated by the Board of Managers; provided, however, that in no event shall any property of the Corporation be transmitted to or vested in any organization other than an organization which is then in existence and then qualified for exemption as a charitable, educational or scientific organization under the Internal Revenue Code of 1954, as amended.

Editor's Note: This Act of Incorporation is shown as amended in 1964 by Francois N. Frenkiel, President, and George W. Irving, Jr., Secretary, acting for the Washington Academy of Sciences, in a Certificate of Amendment notarized on September 16, 1964. A copy of the original Act of Incorporation dated February 18, 1898, appears in the Journal for November 1963, page 212.

JOURNAL OF THE WASHINGTON ACADEMY OF SCIENCES

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Include tables only when the same information cannot be presented economically in the text, or when a table presents the data in a more meaningful way. Consider preparing extremely complicated tabular matter in a form suitable for direct reproduction as an illustration. In such cases, the use of the typewriter is not recommended.

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CONTENTS

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Founded in 1898

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This journal, the official organ of the Washington Academy of Sciences, publishes historical articles, critical reviews, and scholarly scientific articles; proceedings of meetings of the Academy and its Board of Managers; and other items of interest to Academy members. The *Journal* appears four times a year (March, June, September, and December) — the September issue contains a directory of the Academy membership.

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Delegates continue in office until new selections are made by the respective societies.

Mitogenetic Radiation: A Study of Authority in Science[1]

Kurt H. Stern

Naval Research Laboratory, Washington, D.C. 20375

ABSTRACT

Scientists, in doing science, supposedly operate by a procedure known as the "scientific method." Although this method works reasonably well within the structure of a particular science, scientists increasingly fail to apply it as the borders of a science are approached or transcended. This weakness will be illustrated by the history of a relatively recently studied but now almost forgotten phenomenon.

Scientists, particularly when they address the general public and their students, espouse a view of Science which stresses both the impersonal nature of the subject and the high-mindedness of its practitioners. In this aspect Science is seen both as the body of confirmed, existing knowledge and as "scientific method," a proven procedure whereby this knowledge is obtained, a system which is basically independent of the particular scientists involved in it.

Although this view surely has some validity, it is also clear (particularly to scientists) that it omits some important aspects of "sciencing," i.e., the activities which scientists engage in when they *do* science. In fact, these activities themselves became so interesting to a number of scholars that they founded a new discipline, the Sociology of Science. Its purpose is to focus on those aspects of scientific behavior which arise from the fact that it is a human group activity and therefore just as susceptible of study as that of any other social entity. One of the aspects of "sciencing" that has interested me for some time and which I wish to discuss here is the concept of "authority" in Science. In formally authoritarian systems, the meaning of "authority" is quite clear. In the military, in governments, in industry in many church organizations, etc., there exist hierarchies in which commands pass from higher to lower in the chain so that at appropriate places these commands are executed. To the extent that science interacts with government, whether in the present-day context of funding or in the older one of patronage, this authority *over* Science has existed for a long time. (He who pays the piper calls the tune.) However, until their social behavior became itself a subject of study, scientists themselves assumed that there was no authority within science, except for the hierarchical relations between scientists arising from their position within an organization; outside such organizations, scientists were essentially equal. All were free to pursue knowledge and anyone might be right. Even so, it had to be recognized that some

[1] Adapted from the presidential address to the Washington Academy of Sciences, May 1975.

were better scientists than others, and so more prestige, and hence more power, accrued to them. But as long as each scientist is free to pursue knowledge in his own way with access to adequate resources, an egalitarian view remains a reasonable approximation to the real situation. Yet when some scientists control what others shall do because they pass on research proposals or control funding, it is naive to pretend that each scientist is free in his pursuit of knowledge.

Even under the more ideal conditions scientists are not as free as they like to believe. As in other social groups, individual scientists tend to be leaders or followers. At best, leadership is at least partly bestowed on the basis of competence and merit; at its worst, it is largely obtained by contacts with sources of power outside science. At any rate, the pronouncements of "leaders" tend to have a fairly significant effect on the activities of other scientists. It is well known that it makes a difference who authors a particular paper.

We see then, that in science, too, there operates a kind of herd instinct which provides momentum for keeping things as they are (1). Newton's first law translated into the social sphere might read: an accepted idea tends to remain accepted, an unaccepted idea tends to remain unaccepted. This concept is now more usually expressed in terms of Kuhn's "paradigm," i.e., at any particular time in history, phenomena are viewed in terms of some general underlying conceptual framework.

Since threats to the existing paradigms are most likely to come from outside the established science, it is at the boundaries of a science that we are most likely to see the conflict between the professed democratic ideals of science and the authoritarian behavior of scientists who feel threatened by new ideas. For example, the question "what is the melting point of compound X" is one well within a science. Although different investigators may disagree on the particular value, the concept "melting point" is well rooted in science. Therefore scientists are confident that, given sufficient care, disagreements can be resolved and a satisfactory value arrived at.

Other questions, although grammatically equally simple and also within a science, are far more complex. Thus in physics: "is energy continuous or quantized," in geology "do the continents drift," in biology "is cancer caused by viruses," although requiring only a simple "yes" or "no" answer, are composed of a whole complex of questions which must be answered before a simple "yes" or "no" reply can be given to the overall question. At this level of complexity the importance of the paradigm can be clearly perceived. For example, whether energy is seen as infinitely divisible or as quantized has enormous implications not only in physics but in the way in which we perceive the universe. Yet all the above three questions are clearly within Science, and largely even within particular sciences. Hence the solution of these questions, although frequently involving acrimonious controversy, remains discourse between scientists, arguing scientifically. This does not mean that the "right side" necessarily wins (at least initially), as the history of continental drift attests.

A higher level of virulence is reached when the questions occur at the boundaries of Science itself and scientists feel its domain threatened. The reader may test his own reactions by taking his blood pressure while contemplating the following questions: "Is extrasensory perception real?", "Are UFO's real?", "Is Velikovsky right?". Although a study of scientists' responses to these questions (as distinct from answers to them) would provide a great deal of insight into their social psychology, I have no intention of endangering anyone's health. I have therefore chosen to examine the scientific history of a phenomenon which, although not really

outside Science, seemed to be inexplicable in terms of standard science. An additional advantage is that, although the subject in question engendered great controversy and generated a voluminous literature, it is by now so nearly forgotten that hardly anyone will have preconceived ideas concerning it. The phenomenon is known as mitogenetic radiation.

During 1922 and 1923 Alexander Gurwitsch, a Russian cytologist, published several papers and monographs (2) in which he reported a large number of experiments demonstrating the existence of a mitosis-stimulating radiation, soon named mitogenetic radiation. This radiation (MR) had its source in the cells and tissues of the organism and was given its name by Gurwitsch after he became convinced that the stimulus was oscillatory. Gurwitsch's original experiments were done with onion roots (Fig. 1); one root, the so-called sender, was held in a horizontal position close to and pointing directly toward another, the receptor, which was held in a vertical position. After some hours the tip of the receptor was killed, stained, sectioned, and subjected to microscopic examination. It was found that on one side of the receptor there were more cells in the process of division than on the opposite side. This indicated to Gurwitsch that the sender root had radiated some form of energy which accelerated cell division, or growth, in the receptor. To eliminate all possible effects due to volatile oils given off by the onion, and further to ascertain the nature of the emanation, Gurwitsch placed glass and quartz between the sender and receptor roots and found that increase in cell multiplication in the receptor only took place in the case of quartz. This constituted to Gurwitsch a confirmation of his hypothesis that the stimulus was oscillatory and lay in the ultraviolet region of the spectrum, near 200 nm. Although at first ignored, Gurwitsch's work soon stimulated work in many other laboratories. Numerous biological, chemical, and physical investigations

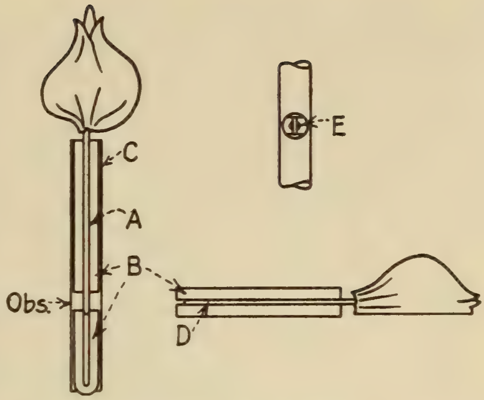

Fig. 1. Arrangement for Gurwitsch's onion-root experiment. A, detector root; B, capillary glass tube; C, metal tube; D, sending root; E, spot marked for sectioning, as seen in observation microscope.

were conducted to study various aspects of the phenomenon. Many cells, tissues, and organisms were said to be sources of MR: bacteria, yeast, hydra, eggs of lower animals, plant seedlings, potatoes, beets, human, frog, and rat blood, cancerous tissue, and others. Even simple inorganic reactions were reported to be sources of MR.

By the early 1930's the literature of MR had become quite large—nearly 600 papers and books—emanating not only from Gurwitsch's laboratory but from research workers all over Europe and the United States. At least part of the reason for this great activity appears to lie in the general interest during this period in the interaction of radiation with biological materials. It was well known that radiation, particularly in the shorter wavelengths, had great effects on cells. It was thus at least conceivable that biochemical reactions in cells might generate radiation. Those who reported positive results continued to work out the characteristics of MR: the radiation generally extends over the range 190–250 nm with extremely low intensity, 10–1000 quanta/cm^2/sec. Each system emits a characteristic spectrum, although related reactions frequently exhibit similar spectra. A selection of spectra is shown in Fig. 2.

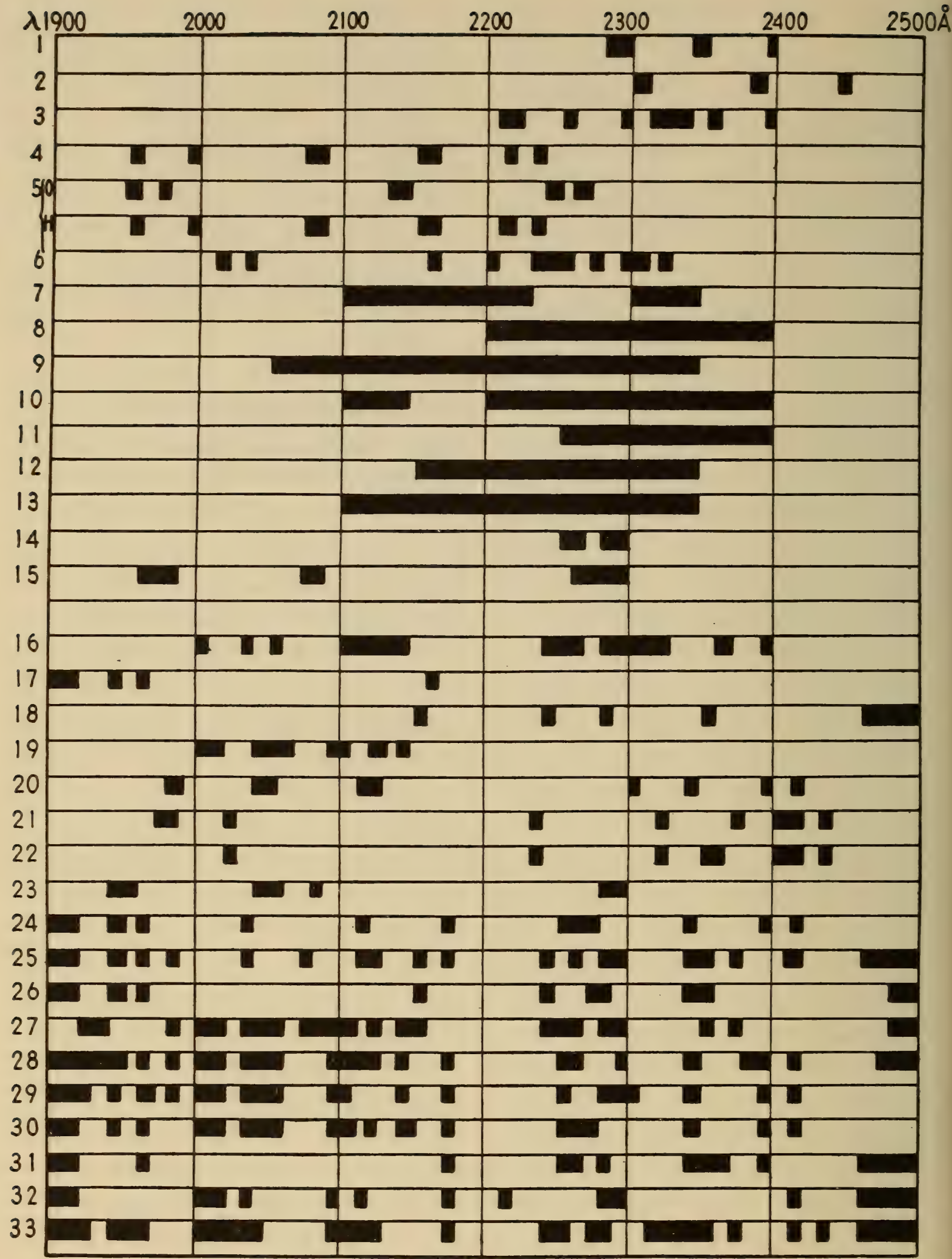

Fig. 2. Mitogenetic spectra. 1. Reduction of Cu″ to Cu (electrochem.). 2. Reduction of Zn″ to Zn (electrochem.). 3. Reduction of Hg″ to Hg (electrochem.). 4. Reaction, HCl + Zn, HCl + Cu, HCl + Mg, HCl + Al. 5a. Reduction of $O_2 \rightarrow O''$ (OH) (electrochem.). 5b. $H \rightarrow H_2$ (electrochem.). 6. Redox reaction, $Fe_2(SO_4)_3 + FeSO_4$ (electrochem.). 7. Reaction, $K_2Cr_2O_7 + FeSO_4$. 8. Reaction, $FeCl_3 + NH_2OH.HCl$. 9. Reaction, $KMnO_4 + H_2O_2$. 10. Reaction, $HgCl_2 + SnCl_2$. 11. Reaction, $HNO_3 + FeSO_4(+H_2SO_4)$. 12. Reaction, $KClO_3$ + Zn + NaOH. 13. Reaction, Pt + H_2O_2. 14. Reaction, KOH + pyrogallol. 15. Reaction, NaOH + HCl. 16. Photosynthesis. 17.

(Cont'd. on following page)

Many workers in the field, however, did not achieve positive results. Consequently, experimenters divided themselves into two groups: on the one hand those, like Gurwitsch himself, who regarded the existence of the rays as established beyond dispute and therefore pursued the ramifications of the phenomenon; and on the other hand those who felt that the phenomenon had never been sufficiently well established and that the fundamental experiments needed to be repeated. In the first category were inevitably some less than competent enthusiasts who failed to bring all relevant variables under control, who reported only positive results, neglected negative ones, and so forth. Positive results, however, were apparently also obtained by careful and competent investigators, but some of these seem to have been insufficiently aware of the importance of statistical analysis.

The problem of detecting such low intensity radiation by physical means was, even in 1936, at the limits of the state-of-the-art. The problems with Geiger-Müller tubes are discussed in a review by Hollaender and Claus (3). Most workers therefore used biological detectors, such as yeast cells and bacteria. However, the biological detectors likewise have their problems. Cells normally divide even in the absence of ultraviolet radiation, so the problem becomes one of deciding how much more mitosis occurs in the presence of MR than in its absence, i.e. the statistical treatment of the data becomes very important. Obviously the shielding of detectors from very low levels of extraneous radiation is also a problem.

Since the intensity of the emitted radiation is very low to begin with and extends over 60 nm, Hollaender (4) estimated that, allowing for the usual losses in the slit system, a system emitting 100 quanta/cm^2/sec would produce 3 quanta in a 10 Å spectral region over a 2-minute period. This radiation is supposed to be enough to cause 0.5 cc yeast suspension to increase its growth by 20%. If this effect is real, it certainly implies a very high sensitivity of living matter to radiation.

In 1934, after a careful review of the existing literature, Bateman (5) summed up the situation as follows: "The unsatisfactory state of mitogenetic literature makes it advisable to regard all that has been written in support of the existence of mitogenetic radiation with the greatest skepticism. The existence of a mitogenetic effect has been neither finally proved nor finally disproved; the evidence against it, though extensive, is as yet insufficient. But supposing that a mitogenetic effect does exist, it is highly improbable that it has anything at all to do with ultraviolet radiation".

In a review written in 1936, Hollaender (4), who had become interested in the problem, blamed Gurwitsch himself for the uncertainty surrounding MR because "he tends pronouncedly to accept the work of investigators whose data agree with his theories and to reject almost entirely criticism of a contradictory nature. The result has been that a large number of scientific workers have become prejudiced against the problem . . . . . . Gurwitsch has not published a clear-cut, detailed description of the methods he has found most successful."

The general interest in the effects of radiation on living organisms is indicated by the establishment in 1928 of a committee of the National Research Council under the chairmanship of B. M.

(Cont'd. from preceding page)

Glycolysis, spectrum from blood radiation (also obtained from corneal epithelium). 18. Phosphatase (action on phosphates). 19. Breaking down of creatin phosphatase. 20. Protein digestion. 21. Reaction, amylase with maltose. 22. Reaction, cane sugar with yeast saccharase. 23. Reaction, urea with urease. 24. From resting nerve. 25. From nerve pulp. 26. Nerve mechanically stimulated (at point of stimulation). 27. Nerve electrically stimulated between electrodes. 28. Nerve, traumatic stimulus about 20 mm. from the trauma. 29. Nerve, electric stimulus 20 mm. from electrodes. 30. Nerve, conduction of stimulus (20 min.). 31. Small brain. 32. Large brain. 33. Optic nerve.

Duggar to support research in this area. A subcommittee on Mitogenetic Radiation, with I. F. Lewis as chairman, and including the physicists L. A. DuBridge (University of Rochester) and F. K. Richtmyer (Cornell) was set up. It decided to provide for an extensive study of the problem by Hollaender at the University of Wisconsin, and to assist other studies by DuBridge and by Rahn at Cornell. It was hoped that close cooperation between physicists and biologists would be fruitful. By 1935, this work was in progress. The 1935–36 report of the Committee on Radiation mentions a grant of $7,500 from the Rockefeller Foundation to support the work of Hollaender and Claus at Wisconsin, and $1,800 to DuBridge and Berry for "an attempt, to establish, if possible, the existence of a stimulatory effect on bacterial cells exposed to ultraviolet radiation." Quantitative studies of growth and metabolism at various levels of intensity were proposed.

By July of 1936 the study by Hollaender and Claus was completed and the subcommittee was discontinued. Their report was published as Bulletin 100 of the National Research Council. In spite, or perhaps because, of extremely careful work, they obtained no evidence for the existence of MR; i.e., all their results were negative. Yet they were properly cautious in not claiming to have *dis*proved MR: "Is the work reported here conclusive enough to be considered incontrovertible evidence of the non-existence of mitogenetic rays? Such evidence has not and cannot be produced by any investigation, since it is logically not possible to prove that a phenomenon as described by Gurwitsch does not exist. . . . While we recognize the hopelessness of the present situation in reference to the mitogenetic problem, we shall not hesitate to start work again on it if "successful" investigators fulfill the conditions as we have outlined them. We should not hesitate to continue this work; in fact we should hope to be given the opportunity in the future to return to this problem if we felt that the evidence warranted another attack on it."

In spite of the carefully drawn distinction between proof and disproof, the effect of the report was profound. Publication in this field virtually ceased. Since I have no evidence that publication of further work was prevented or suppressed, it seems most likely that we are simply seeing the voluntary acceptance of authority by scientists. The reasoning is simple: if all this careful work could not prove the existence of the phenomenon, how could I hope to do better? Even if I did, would my work be accepted or would I just be jeopardizing my career?

Nevertheless, in 1943 the Faraday Society sponsored a symposium at which MR was discussed. The French photochemist Audubert (6) reported that ultraviolet radiation of the intensity claimed for MR was emitted by a variety of inorganic reactions and had been detected by purely physical methods. Gurwitsch (7) presented a paper in which he claimed that as a result of photochemical work the physical basis for MR had been firmly established. Previously, one of the conceptual dilemmas had arisen from the fact that the appearance of ultraviolet photons, corresponding to energies of 150 kcal, was inconsistent with the overall energies of the reactions which were much lower. Gurwitsch now argued that the observed energies could only arise from the combination of free radicals which must be present at very low concentrations. Although these papers appeared to offer both a plausible explanation for MR and a number of hypotheses susceptible to experimental investigation, their impact in stimulating further work seems to have been negligible.

Nevertheless, although research on MR in the Western countries apparently ended after 1943, Gurwitsch and his collaborators continued work at a laboratory in Moscow. In 1959, after the death of A. G. and L. D. Gurwitsch, a book authored by them (8) was published in East Germany. In the intro-

duction to this book, which contains a great deal of experimental information, the authors state that the results of MR are not easily fit into standard science. They therefore considered two tasks possible for workers in the field: (a) to develop theories consistent with both "classical" biology and with MR. This is desirable, but beyond the powers of the investigators. (b) Perform studies within the framework of MR which are not contradicted by the *exact* results of classical biology; the explanations developed must be testable. The nature of the "non-classical" results is described as follows: a number of micro-events of chemical, biological, and physical nature can be detected only by non-classical techniques. They largely consist of statistically improbable events, e.g., the absorption of photons by only a few molecules at the beginning of a chain reaction leading to important events in cell division.

Not surprisingly, the authors criticize the work of Hollaender and Claus (although they admit it is carefully done) on the basis that previously untested bacteria were used, that the cultures were not aged sufficiently, and that the exposure time was too short. They insist that negative results are only convincing if it can be shown that positive results have been incorrectly interpreted.

In a review of the book, published in 1960 (9), Hollaender expresses surprise that MR is still with us and attributes this to the attractiveness of the phenomenon (if it existed) and to Gurwitsch's "integrity, imagination, and drive" which persuaded Soviet authorities to support his work for so long. He feels that the reason for Gurwitsch's failure to induce others to take up work on MR lay in the failure of others to repeat Gurwitsch's results. However, this overstates the case since in fact many others did reproduce Gurwitsch's results, although Hollaender himself (and others equally competent) did not.

We have here an interesting dilemma in scientific methodology which may be summarized as follows: Gurwitsch: If positive results cannot be shown to be the result of incorrect experimentation or interpretation, they must be regarded as valid. Negative results only show that the experimenter did not perform the same experiment (no matter what he thought he was doing) and therefore they do not invalidate the positive results. Hollaender: It is logically impossible to establish the non-existence of a phenomenon, but the repeatability of experiments is at the essence of science, and if many investigators cannot repeat an experiment, this may not logically disprove the existence of the phenomenon but one must not be surprised if they cease working on the problem.

Both positions are defensible, but historically it is obvious which won out. Since there were many investigators on both sides of the issue, it is clear that the authority of some carries more weight than that of others. This kind of authority is very different from the authority actively exercised by an individual. Rather, it flows to him from a community which attributes certain desirable qualities to him. The result is that such an individual's judgment, whether right or wrong in a particular instance, will be given great weight in forming the perceived reality and may outweigh that of a much larger number of persons not held in such esteem. In other fields this phenomenon is well known and described by the term "opinion leader." Yet, although scientists pride themselves on their independence of thought, my little history (and other examples I could equally well have chosen) show that *even when doing science*, scientists do not behave so differently from what has been observed for human group behavior generally.

We must now ask whether the kind of situation I have described can be eliminated or, at least, whether the dependence on authority can be lessened. Given the natural proclivities of human beings, the answer is not encouraging. Probably the best we can do is to make sure that such dependence is

voluntary, rather than that it arises from the stifling of opposing views. Recently there have been complaints about the proliferation of journals and the difficulties of keeping up with the literature in various fields. However, this proliferation does serve to facilitate access to the scientific community even by those whose findings are controversial, since it is unlikely that a given work that has some merit will be turned down by every journal. Thus a multiplicity of communication channels fosters scientific democracy.

Aside from providing for maximum communication of ideas between scientists, there is a more personal duty that obligates every scientist to examine himself for bias and dogmatism. This point was expressed most strongly by Jacob Bronowski in his TV series "The Ascent of Man": the greatest fault in science is to be totally certain. In science, as in politics, it leads to totalitarianism and inhumanity.

Although it has not been my purpose to decide whether MR exists, but only to use it as an illustration of some aspects of authority in science, it is interesting to speculate on a possible future for the subject. In 1954 Commoner, Townsend, and Pake (10) reported detecting (by paramagnetic resonance) low concentrations of free radicals in a wide variety of biological tissues. These radicals are associated with metabolic activity in both plant and animal material. Since then, the importance of free radicals in biological systems has become well recognized. For example, the biological degradation of molecular oxygen involves superoxide, perhydroxyl, and hydroxyl radicals, and may give rise to some kinds of bioluminescence (11). Similar studies on inorganic systems have, to my knowledge, not yet been carried out. Whether any of these reactions give rise to low-intensity ultraviolet radiation is a question which has not yet been answered. Certainly the physical detectors available now are far more sensitive than those of forty years ago. It would be interesting if, as a result of my historical and sociological musings, someone were stimulated to devise at long last a critical test for mitogenetic radiation.

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National Mosquito Control—Fish and Wildlife Coordination Committee: A Status Report[1]

Claude H. Schmidt

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ABSTRACT

The purpose of this committee, created in 1960 in Washington, D. C., is to establish mechanisms that will stimulate and promote mutual objectives and closer working relationships between mosquito control and wildlife conservations interests. Over the past fifteen years, the committee has striven to meet this mandate.

All too often people communicate only with others in their own interest group, although there is a real need to expand communications to include other disciplines, especially those that are related. The history of the National Mosquito Control—Fish & Wildlife Coordination Committee is a case in point. It was established to provide mechanisms that would stimulate and promote mutual objectives and closer working relationships between mosquito control and wildlife conservation interests. So here is some background on this interdiscipline group, including a report on what has been accomplished over the past 15 years and what is being done now.

In the late fifties, there was little cooperation and even less coordination between mosquito control and wildlife management groups. Each did its own thing to accomplish its objectives and had little or no concern about the interest or problems of the other. In fact, at times these groups worked at cross purposes. For example, an area would be drained to control mosquitoes and would thereby be rendered useless for fish and wildlife; or impoundments for fish and wildlife would be created, and there would be hordes of mosquitoes as a consequence. It became evident, as one member of our committee (Vannote, 1971) so aptly put it, that "Coordination cannot exist until there is mutual understanding of common problems. And there must be mutual trust established between parties—and the establishment of mutual trust between parties can only be obtained by approaching common problems in good faith." How could this concept be fostered and implemented? Well, two men of foresight, Dr. Paul Springer of the Fish and Wildlife Service and Mr. Robert L. Vannote of the American Mosquito Control Association, got together and discussed the need for coordination and cooperation between fish and wildlife workers and mosquito workers, especially in situations where the interests of both were involved. By using these discussions as a springboard, these two men were able to promote the idea of coordination that resulted in a symposium on "Coordination of Mosquito Control and Wildlife Management" held in April 1959 in Washington, D. C. One of the resolutions adopted at that meeting is as follows (Springer and Vannote, 1961):

"WHEREAS it is the opinion of this Symposium that coordination between mosquito and wildlife conservation is most desirable; now therefore, be it

"RESOLVED, that this symposium recom-

[1]Based on a talk given at the annual meeting, Entomological Society of America, Dec. 3, 1974, Minneapolis, Minn.

mends the formation of a national joint committee consisting of three members representing wildlife conservation; and three members representing the mosquito control interests, consisting of one member of the U. S. Department of Agriculture, one member of the U. S. Public Health Service, and one member from the American Mosquito Control Association; and be it further

"RESOLVED, that this committee organize as soon as is practicable after approval of the Federal interests involved, for the purpose of investigating the avenues of coordination between mosquito control and wildlife conservation, particularly in respect to research and operation."

In accordance with the resolution, the following persons were designated in 1959 by the government agencies and wildlife conservation groups to form the committee:

Dr. Paul F. Springer, U. S. Fish and Wildlife Service, Secretary
Dr. Ken A. Quarterman, U. S. Public Health Service
Dr. A. W. Lindquist, U. S. Department of Agriculture
Mr. Robert L. Vannote, American Mosquito Control Assoc., Chairman
Dr. Ira Gabrielson, Wildlife Management Institute
Mr. Elwood A. Seaman, American Fisheries Society

Nine months after the resolution was passed, January 29, 1960, the first meeting of this committee took place in Washington. At the first meeting, an important item of business was the name of the group. Even today, many people, when they first hear about the committee, wonder about the very long name. After much discussion, it was concluded that every word was significant and that none of the words could be left out if the name was to convey the true nature of the committee's charge. The long title was approved, National Mosquito Control—Fish and Wildlife Coordination Committee.

With the name settled, the committee drew up the following list of objectives (Springer and R. L. Vannote, 1961):

1. To coordinate mosquito control and fish and wildlife management policies on national, state, and local levels.
2. To gather and disseminate relevant information and suggest standards on mosquito control techniques consistent with sound fish and wildlife management objectives.
3. To gather and disseminate relevant information and suggest standards on fish and wildlife management techniques consistent with sound mosquito control objectives.
4. To stimulate needed research and demonstration projects relating to mosquito control and fish and wildlife management practices.
5. To sponsor suitable meetings to further the purposes of this committee.
6. To cooperate with agencies, organizations, and all others whose activities may relate to those of this committee.

The 6 members of the committee had their job cut out in implementing these objectives, and the work was in addition to their other full-time duties. They felt the best way to begin was to prepare a special brochure to inform all interested parties about the membership of the committee and the objectives. The brochure also contained an invitation for suggestions on how the committee could best serve the mutual interests of the two other groups.

The next project the committee tackled was a questionnaire that was sent out to all mosquito abatement districts and wildlife management working groups. They were requested to list, in priority order, the most important problems facing the respective groups. The response was excellent, and over 100 questionnaires were returned. The committee also wrote to all the fund granting agencies in the United States suggesting that it would be in the public interest and would also further the aims of the committee if (Quarterman, 1962) "such projects not be approved until they had been expanded to include a joint approach by representatives of both groups in cases where it was obvious that a joint approach was needed in order to find a solution which would be usable. We should not spend public monies or research grant monies in an attempt to solve a problem only to find when we have apparently solved it from one point of view, a new problem results for another interest, which makes it impractical to use that apparent solution. The reason-

able procedure is to work out jointly a solution to the problem so that it will be usable by both groups." That statement aptly expresses the basic philosophy of the committee.

The committee also wrote (Quarterman, 1962), "To all of the state fish and game commissions, state health departments, and state departments of agriculture, to universities which had entomology or wildlife departments that might be involved in either sponsoring or applying for a research grant to solve a problem in this field, again pointing out the necessity of approaching the problems as joint undertakings by the two groups."

For the first 2 years, the committee's activities were quite extensive. The stamps, letterhead stationery, questionnaires, and brochures were paid for by the contributions from many fish and wildlife societies and mosquito control groups. And every time the committee needed additional operating funds, they were obtained from both interests.

The membership of the committee remained at 6 until 1971 when a new governmental unit, the Environmental Protection Agency, was formed. Since the aim of that agency reflected many of the objectives of our committee, we invited William D. Ruckelshaus, the Administrator of EPA, to designate a representative. This was done, and Thomas Devaney joined our group. Now we have 7.

Let us take a look at the track record —what has been accomplished.

During 14 years, from 1960 to 1974, 8 meetings of conferences were cosponsored by our committee as follows:

1. Conference for Coordinated Program on Wildlife Management and Mosquito Suppression, Yosemite National Park, October 15–18, 1962.

2. 2nd Yosemite Conference—Coordinated Program on Wildlife Management and Mosquito Suppression, Yosemite National Park, May 3–6, 1964.

3. First Gulf Conference on Mosquito Suppression and Wildlife Management, Lafayette, Louisiana, 1964.

4. Northeastern Conference on Mosquito Suppression and Wildlife Management, Newton, Massachusetts, April 20–22, 1966.

5. Southeastern Conference on Mosquito Suppression and Wildlife Management, a joint meeting with the Florida Anti-Mosquito Association, Cocoa Beach, Florida, October 21–23, 1970.

6. 2nd Gulf Coast Conference on Mosquito Suppression and Wildlife Management, New Orleans, Louisana, October 20–22, 1971, held in conjunction with the annual meetings of the Louisiana Mosquito Control Association and Gulf States Council on Wildlife Fisheries and Mosquito Control.

7. Joint meeting with Northwest Mosquito and Vector Control Association, Eugene, Oregon, October 31 to November 2, 1972.

8. Joint meeting with the North Central Region —American Mosquito Control Association and the Illinois Mosquito Control Association, Chicago, Illinois, March 25–26, 1974.

These regional meetings provided a place where mutual problems could be discussed freely and dispassionately. There was an additional advantage in holding them in different geographical areas because the participants and the committee members could get a first-hand look at local problems and how they were being resolved. Some of the proceedings of these meetings are still available, especially the more recent ones.

There was a secondary, longer-lasting effect from at least 4 of these conferences and meetings—the formation of state and regional coordination committees patterned after the national committee. These organizations were apparently sparked by the comment of Dr. Quarterman at the First Yosemite Conference (Quarterman, 1962): "Perhaps this is the time when you would want to consider organizing a coordinating committee at the state level, certainly in California, perhaps in other nearby states such as Utah, where there is considerable mosquito control and wildlife management activity going on. Such a committee could meet at regular intervals and consider mutual problems and try to organize joint approaches to the solutions. I think you would find that this would go a long way towards solving problems that affect both groups."

The Utah delegates got the message. As soon as they got back home, the Utah Mosquito Control—Fish and Wild-

life Management Committee was formed. It consisted of representatives of three agencies: Don M. Rees, Division of Zoology and Entomology, University of Utah; Donald A. Smith, Waterfowl Supervisor, Utah Department of Fish and Game; Jessop B. Low, Utah Cooperative Wildlife Research Unit, Bureau of Sport Fisheries and Wildlife. This group has been very active in sponsoring meetings and field trips and several demonstration projects of great importance in wildlife management and mosquito control.

The California group was not far behind. They organized their coordinating committee following the 2nd Yosemite conference in 1964. This committee was composed of representatives of 5 organizations: The California Mosquito Control Association, The California State Department of Fish and Game, The California Department of Public Health, The University of California, and The U. S. Fish and Wildlife Service. Mr. Oscar V. Lopp, the representative of the California Mosquito Control Association, played a key role in the formation of this group and served as its first chairman.

The third group, the Northern Gulf Coordinating Council, was formed in Louisiana in 1965 as a result of the First Gulf Conference on Mosquito Suppression and Wildlife Management. The chief purpose was to create friendly relations among wildlife, fisheries, and mosquito-control interests. This group has stressed the concept of coordination and has expanded it to include a multitude of cooperative efforts with rice farmers, cattlemen, shrimp and crawfish farmers, and landowners.

A fourth group was formed as the result of the Northeastern Conference on Mosquito Suppression and Wildlife Management held in Massachusetts in 1966. Mr. Robert W. Spencer, president of the Northeastern Mosquito Control Association, had an active role in this endeavor.

The National Mosquito Control—Fish and Wildlife Coordination Committee, in addition to helping to form local and regional groups, has provided much information to the public through answers to the many inquiries it has received over the years. For example, in the spring of 1968, problems developed in Toledo, Ohio, over the use of fenthion formulated as Baytex^R, an organophospate insecticide that had been substituted for DDT 4 or 5 years before. Much controversy had developed as a result of several newspaper articles. Because of the problem, the Toledo Area Sanitary District in cooperation with the U. S. Fish and Wildlife Service ran several tests to evaluate the aerial application of fenthion in an area west of Toledo. Mr. Robert Vannote represented our Committee at these tests.

In 1970, the committee decided to revise the brochure prepared in 1960 because the situation had changed during the intervening years. As an example, there was now a great deal of interest by the public in all things environmental. The point was made in the revised brochure that on the one hand, our health must be protected, but on the other, ecological significance of mosquitoes and the methods of controlling them must be well understood. Also our environment must be protected against the potential hazards of pesticides, biological control agents, and other nonchemical methods of mosquito control; so the benefits and risks must be carefully weighed before decisions are made and programs implemented. When chemicals are used, water levels altered, or certain wildlife management practices followed, the environment for fish, birds, insects, and other beneficial creatures in the treated area rarely remains undisturbed. Consequently the application of management procedures by personnel who have not carefully anticipated possible detrimental consequences can stimulate controversies. Thus managers of each specialized activity, be it mosquito control or fish and wildlife management, should have some appreciation of the objectives of the others. This points out the need for guidelines for field applications.

Our current project is the development of a set of broad guidelines for effective mosquito control which is being prepared under the direction of Dr. Frank Murphy, who represents the American Mosquito Control Association on the committee. We hope to have this in print within 6 months.

The present representation on the committee is as follows:

James W. Akerman[1]	Environmental Protection Agency
Lawrence R. Jahn	Wildlife Management Institute
Jerry R. Longcore	Fish and Wildlife Service, U. S. Department of Interior
Frank J. Murphey	American Mosquito Control Association
Claude H. Schmidt[2]	Agricultural Research Service, U. S. Department of Agriculture
James V. Smith	Center for Disease Control, U. S. Department of Health, Education and Welfare
Richard A. Wade	American Fisheries Society

[1]Secretary
[2]Chairman

Over the years this committee has had a useful function. And during the past 5 years, there has been a great intensification of public concern over the environment. I believe that the committee has previously and will now respond to this concern by providing a forum where problems of mutual interest can be discussed.

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Some Aspects of the Early History of Penicillin in the United States

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ABSTRACT

When Nobel laureate, Sir Howard Walter Florey, and his associate, Dr. Norman Heatley, came to the United States from Oxford, England in 1941 seeking help in the production of a sufficient quantity of penicillin for conclusive clinical evaluation, they were directed through a chain of persons to USDA's Northern Regional Research Laboratory. There the problem of penicillin production was essentially solved in a remarkably short time by the discovery that corn steep liquor greatly enhanced penicillin yields. The story of these bits of penicillin history is recounted.

The story of penicillin has many interesting facets and much has been written about it and the principal people who participated in its discovery and development into one of man's greatest weapons against infectious diseases. The name of Fleming, I suppose, will always be remembered as the discoverer of penicillin as indeed he was. Curiously, he failed to follow up on his momentous discovery, and it lay dormant in the scientific literature for almost a decade until Dr. Howard Walter Florey and his group of researchers at Oxford University in England carried out their studies on antibacterial substances. Among these was penicillin. It is to Florey's genius, then, that we owe our thanks in establishing the clinical usefulness of penicillin in treating many of the common infectious diseases. We owe him further for his inspiration in coming to the United States at the right time and finding the kind of help he needed to bring his discovery to complete fruition.

It was my good fortune to play a small role in the penicillin drama. In *my* book Dr. Florey was one of the world's great benefactors and I shall always be grateful for the opportunity I had for helping this great man fulfill his dream.

It was indeed a miracle that Florey ever found the route to success here in the United States in making penicillin available economically in quantities adequate for large-scale use in medicine. In football parlance it required a quadruple pass with no fumbles for the ball to reach me and then one more pass for me to get Florey's problem to the one place in the world where the job could best be done. That place was the Northern Regional Research Laboratory of the United States Department of Agriculture in Peoria, Illinois. This whole "series of plays" occurred in a remarkably short

period of time, considering the geography involved. From Oxford, England to New York. From New York to New Haven. From New Haven to Washington, D. C. And finally from Washington to Peoria.

None of the participants knew that by the time Florey's problem reached Peoria the "game" was won. The background and details of this fantastic story are well told by Lennard Bickel in his biography of Florey (1). The episode that occurred in the United States is described in Chapter 11 of this book. Since my part remains crystal clear in my memory, I want to set down some additional details that may be of interest.

At the time of Florey's visit to the United States in 1941 I was Director of USDA's Eastern Regional Research Laboratory with headquarters in Wyndmoor, Pa., a small suburb of Philadelphia. How did it happen that I was in Washington, D. C. on July 9 of that year when Dr. Florey and his associate, Dr. Normal Heatley, were brought to my temporary office by Dr. Charles Thom, world famous mycologist, of USDA's Bureau of Plant Industry? I have to confess that I was there that afternoon much against my will and thereby hangs an interesting detail of this story.

When USDA's four Regional Research Laboratories were authorized by the Congress in February, 1938 for the purpose of developing new uses for farm products, Mr. H. T. Herrick, as head of our Bureau's Industrial Farm Products Division, became deeply involved in the programs and plans for these new institutions. Eventually he was promoted to the position of Assistant Chief of the Bureau of Agricultural and Industrial Chemistry and the operations of the four new laboratories were placed under his direction.

It so happened that Mr. Herrick had a great passion for traveling and in his new position found much need and opportunity for exercising this urge. Eventually our Chief, Dr. Henry G. Knight, imposed the restriction that he would have to be at his Washington, D. C. headquarters long enough to perform his pressing duties there. Herrick was chastened but undaunted by this new directive and he set about to arrange things so that he could pursue his travels. He not only hatched a scheme to accomplish this but also succeeded in having Dr. Knight issue orders putting the plan into effect.

The plan was simple and direct. It required each of the four Regional Laboratory Directors to spend two months each year in Washington, D. C. These tours of duty were to last for one month and the Directors were to serve in rotation. From Mr. Herrick's point of view the plan was perfect—it would afford him a cover for his job there in Washington during eight months of the year and would permit him to travel at will. I was the first to be "hooked" for this duty and my first service under the plan was, during July 1941.

Accordingly I reported for duty in Washington on July 1, and sure enough, Mr. Herrick promptly left on an extended trip. Thus I was sitting in his chair on the afternoon of July 9 and serving as acting Assistant Chief of our Bureau. I wasn't very happy over this turn of events.

On the afternoon of July 9 Carl Speh, who headed our Bureau's enlarging defense activities, had called a meeting for 2 pm. Shortly after 1:30 the corridor door opened and into my office walked Dr. Charles Thom, whom I knew well, with two men in tow. He introduced his guests as Dr. Howard Florey and Dr. Norman Heatley. They were from Oxford, England and they had urgent business relating to the production of penicillin by Fleming's *Penicillium notatum*. Dr. Florey was the spokesman and he quickly summarized his work, his hopes and his needs.

As I listened it seemed to me that the solution to his problem was clear. He wanted to make a quantity of penicillin by mold fermentation to extent his clinical studies. It so happened that mold fermentation was my special field of work as it has been from 1930 until 1939 when I left to assume charge of the Bureau's

Eastern Regional Research Laboratory. Our fermentation group, formerly located at the Bureau's Color Laboratory on the Arlington Experimental Farm across the Potomac River from Washington, D. C., had been transferred to our Northern Laboratory in Peoria, Illinois when that facility was completed late in 1940. There we had excellent arrangements for this kind of work and many years of research experience in both mold and bacterial fermentations. My mind was made up. Florey and Heatley should go there for help. It was not a difficult decision at all. The matter of low penicillin yields didn't faze me. Improving yields of products was one of our principal objectives in every fermentation we studied and we generally succeeded.

We discussed the matter briefly and I explained my thinking. Dr. Florey readily accepted my decision. Immediately I began writing a telegram to my friend and close associate, Dr. Orville E. May, Director of our Bureau's Northern Regional Research Laboratory explaining the mission of Florey and Heatley and asking if certain equipment was available. Our master machinist Rudolph Hellbach had several years earlier constructed a pilot type shallow pan aluminum fermenter which I knew would be useful, but I was aware from our experiences at Wyndmoor in starting up our operations that the fermenter might still be uncrated after its transfer journey from the Color Laboratory to Peoria. I recall now with some amusement that I interrupted my writing of the telegram to check with Dr. Florey on the correct spelling of his name. He nodded to indicate I had it right. I completed the wire to Dr. May and told our visitors that I would have an answer for them the following morning and suggested to Dr. Florey that he call me about ten o'clock. We shook hands and my three visitors left the office. They had come, of course, to see Mr. Herrick but found me there instead. I looked at the clock. It was exactly 2 PM. I gave my long-hand telegram to Mr. Herrick's secretary and sped off to our Bureau's Defense Committee meeting, not realizing what important events of the future had just been brewed.

My message of July 9 to Dr. May and his reply that same day were terse communications. Mine read "Thom has introduced Heatley and Florey of Oxford. Here to investigate pilot scale production of bacteriostatic material from Fleming *Penicillium* in connection with medical defense plans. Can you arrange for shallow pan setup to establish laboratory results." His reply read "Pan set and organisms available Heatley and Florey experimentation. Details of work, of course, unknown. Suggest they visit Peoria for discussions. Laboratory in position to cooperate immediately."

The following morning Dr. Florey called me from his hotel in Washington and I was able to tell him that all was in readiness for their visit. He told me they would leave Washington Sunday evening and would expect to arrive in Peoria at about noon on Monday, July 14. They arrived as planned and were engaged in discussions that same afternoon with Dr. May and his associates at the Northern Laboratory. Momentous decisions and plans were made that day. Dr. Florey produced his culture of the organism they had been using at Oxford to make penicillin. It was agreed that Dr. Heatley would stay on at the Northern Laboratory to assist in the work and to teach the NRRL personnel their method of penicillin assay. That tour of duty for Heatley lasted a whole year.

Regretfully I never saw Florey or Heatley again after our meeting on July 9. As I write this in 1975 it seems a bit strange that Florey accepted so quickly my proposal for them to go to the Northern Laboratory. After all, he was a free agent and had other places in mind where he would seek help. Why should he have accepted without hesitation? I can only surmise that it was the prestigious people that brought him and Dr. Heatley to us— Florey's friend John Fulton in New Haven, Connecticut; Prof. Ross Harrison, President of the National Academy

of Science; Dr. Charles Thom, world famous USDA mycologist. I might have had some influence too. I know I was very confident that our Northern Laboratory associates could help them. Florey later wrote of our meeting as being "very friendly." Whatever the reasons, they were part of the miracle that Dr. Robert Coghill, leader of the penicillin project at the Northern Laboratory, wrote about many years later (2). He said in part, "Penicillin has often been referred to as a miracle drug, but one of the least understood miracles connected with it is that Florey and Heatley were directed to our Peoria laboratory."

In writing this Dr. Coghill had something else in mind. He went on to say "I do not say this because I feel that we were smarter or knew more than other fermentation people or had a better understanding of the penicillin problem, but because it was, I am sure, the *only* laboratory in the country where the corn steep liquor medium would have been discovered. Moreover, with us it was no flash of genius, as has sometimes been suggested, but a simple routine procedure. We had tried corn steep liquor in every fermentation problem we ever studied. The discovery of its key place in a penicillin medium was foreordained and inevitable once the problem was assigned to our Fermentation Division."

I have to agree with Dr. Coghill that getting Florey and Heatley to the Peoria laboratory was indeed miraculous. It was their physical presence at the place where the answer to the penicillin production problem existed long before they arrived. It took a kind of fate to bring the question and answer together. In the research work at Peoria it developed that corn steep liquor added to the culture medium in the right amount was the key to success in making penicillin. The background of the story of its use by nutritional mycologist Dr. Andrew Jackson Moyer precedes the very existence of the Northern Regional Research Laboratory.

The story goes back to 1925 when the team of Herrick and May began the mold fermentation studies at the former Color Laboratory of USDA's Bureau of Chemistry. The mold and bacterial fermentations were studied there, leading to the production of gluconic acid, citric acid, kojic acid, lactic acid, the keto-gluconic acids, and l-sorbose. There was assembled the group of chemists and mycologists that was to play such a crucial role in the penicillin story. There we researched under the inspiring leadership of Dr. Orville E. May—the same Dr. May to whom I sent my telegram on July 9, 1941. There was gathered the knowledge and skills necessary for Coghill's miracle—surface and submerged fermentations and the equipment to make them succeed, and special nutrients such as corn steep liquor.

By 1936 when Dr. May left the Color Laboratory to become Director of the newly established Regional Soybean Research Laboratory in Urbana, Illinois, we were approaching the crux of the gluconic acid fermentation study. Success came slowly, and until we learned how to make the rotary fermenters operate continuously and without attention, there were many months of frustration. It is a matter of record that Dr. May, Dr. George Ward and I babied our rotaries around the clock for a long period until we achieved a method of operation that permitted repeatable experiments. Then the dam broke and over a short period we determined all the factors necessary for a very rapid process with high yields of product.

Our next step took us to a much larger rotary fermenter that we had constructed and placed in operation at our Bureau's Agricultural By-products Laboratory on the campus of Iowa State University at Ames, Iowa. This enterprise was my responsibility and along with A. J. Moyer and others we made it succeed during the period November 1936–March 1937. We called it a pilot-plant, and indeed it was when compared with our small rotary laboratory-scale fermenters. This new fermenter had a working capacity of 530 l as compared to about 3 for the small ones. The figure 530 is mentioned, since it had

an important bearing on the discovery of corn steep liquor as a microbial nutrient.

Following the gluconic acid work at the Ames Laboratory we undertook intensive study of the sorbose fermentation at the Color Laboratory in Arlington, Virginia. This rare ketose sugar had assumed considerable commercial importance, as it was the key intermediate in the synthesis of Vitamin C by the Reichstein method. Our earlier know-how hastened this new study and soon we established the conditions by which 20% sorbitol solutions could be fermented to l-sorbose with nearly 100% yields in less than 24 hours. This represented a great advance in the art. Our next step, of course, was to pilot-plant the process in our large fermenter at Ames, Iowa. Capacity 530 liters!

One day I sat down to determine the materials required for this large-scale study. Our nitrogen source for the fermentation was Difco Yeast extract. It cost about $5 a pound, but for our laboratory-scale experiments this was of little consequence. We required 5 g/l of culture solution, and a quick calculation revealed that the cost of this material for each experiment at Ames would be about $30. In these days of multi-million dollar budgets that cost would be insignificant, but in those days we were literally poor. And a good thing too!

I talked to our team about the problem and suggested that we try to find a lower cost substitute for the Difco Yeast extract. We tried many waste fruit and vegetable juices and found that some of these were promising. For example, both cabbage and tomato juices gave good sorbose yields. Then some one of us found a gallon bottle labeled "corn steep water concentrate". A brushing removed the dust and revealed that it came from the A. E. Staley Manufacturing Company in Decatur, Illinois. It was recalled then that on one of his numerous trips Mr. Herrick, together with Dr. May, had visited the Staley research laboratory. The people there told them about their problem of finding uses for a by-product obtained from the manufacture of corn starch by the wet milling process. It was the waste steep water which they had available as a concentrate in huge quantities. Herrick and May were thereupon made the recipients of a gallon bottle of the concentrate. They brought it back, discussed it with us, then it was relegated to a corner of the laboratory to collect dust and await its fate a few years hence. Corn steep liquor looks much like molasses. It contains a host of organic and inorganic substances leached from corn during the steeping process.

We compared it with Difco Yeast extract and obtained identical results when we used 3 g of the steep liquor concentrate per liter. With the addition of an anti-foaming agent and making a minor adjustment in pH, we had a perfectly satisfactory solution to our problem. Corn steep liquor cost about $0.10 per pound even in small amounts! This was one of Mr. Herrick's trips that paid large dividends.

This work was done about mid-1937 but it was not until December 1939 that the results were published (3). Our associate, Dr. A. J. Moyer, was not a member of our sorbose team but he worked in the same laboratory with us and was thoroughly familiar with our finding about the usefulness of corn steep liquor as a microbial nutrient. Therefore it was natural for him to try corn steep liquor when he began his studies on the penicillin fermentation at the Peoria laboratory during the summer of 1941 after the visit of Florey and Heatley. Presto! The effect was astonishing and magical. In the optimum concentration it multiplied the penicillin yield many-fold and remains today the key factor in the industrial production of this antibiotic.

Although Dr. Moyer knew very well about corn steep liquor as a nutrient, nevertheless he deserves full credit for being the one who discovered its usefulness in the penicillin fermentation and thus contributed mightily to this great development.

It was a timely discovery because

industrial interest was lagging until the information about the corn steep liquor results became available. More importantly, this discovery together with other improvements in the process made penicillin available for massive use in treating battle casualties at the time of the Normandy invasion in June 1944. This new drug undoubtedly saved thousands of lives during the latter part of World War II and many more since that time.

The success of penicillin furthermore touched off many successful searches for other needed antibiotics. Thus penicillin was one of the great medical advances in the first half of the twentieth century.

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(2) Coghill, R. D., Chemical Engineering Progress Symposium Series, *66*, 18, 1970.

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Announcement

The VISITING LECTURER PROGRAM IN STATISTICS is continuing into its thirteenth successive year. The program is sponsored jointly by the principal statistical organizations in North America—the American Statistical Association, the Biometric Society and the Institute of Mathematical Statistics. Partial support is also provided by the International Business Machines Corporation. Leading teachers and research workers in statistics—from universities, industry and government—have agreed to participate as lecturers. Lecture topics include subjects in experimental and theoretical statistics as well as in such related areas as probability theory, information theory and stochastic models in the physical, biological and social sciences.

The purpose of the program is to provide information to students and college faculty about the nature and scope of modern statistics, and to provide advice about careers, graduate study, and college curricula in statistics. Inquiries should be addressed to: H. T. David, Visiting Lecturer Program in Statistics, Department of Statistics, Iowa State University, Ames, Iowa 50010.

Among the participating lecturers in this area are: KALI S. BANERJEE, University of Delaware; JEROME CORNFIELD, George Washington University; CHURCHILL EISENHART, National Bureau of Standards; SAMUEL W. GREENHOUSE, George Washington University; THOMAS B. JABINE, Department of Health, Education, and Welfare; DONALD JENSEN, Virginia Polytechnic Institute; FRED C. LEONE, American Statistical Association.

RESEARCH REPORTS

A New Species of Shield-Backed Katydid from Cerro Aconcagua, Argentina, with Notes on Other Species and Their Habitats (Orthoptera, Tettigoniidae, Decticinae)

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ABSTRACT

Platydecticus anaesegalae, n. sp., is a wingless, mostly black decticine which varies from 6 to 13 mm in length and was collected at an altitude of 4,250 m. on Cerro Aconcagua, Mendoza, Argentina. The closest known relative is *P. angustifrons* Chopard from Neuquén, Argentina.

Only a few species of Decticinae have been reported from South America, though they are abundant in North America, so we feel privileged to study and report another species, this one from the highest South American mountain. Notes on other high-altitude Orthoptera are included.

Comparatively little information on South American Decticinae has been published. Bruner (1915: 398), referring to Caudell's 1908 review of the world decticine fauna, believed that there were no South American records of Decticinae. However, Caudell (1908: 1,23) noted a single species that was described as *Decticus fuscescens* by Blanchard (1851: 44) from Coquimbo, Chile. Caudell placed it questionably in the genus *Tettigonia*, using the genus in the sense of the current genus *Decticus*. Uvarov (1924: 527) said that *fuscescens* has "nothing to do with this purely Palearctic genus." Nevertheless, the name *Tettigonia* was retained for it by Piran (1941: 135). It seems clear that the true generic position of *fuscescens* remains to be clarified, though the species is fully winged and is a quite different insect from *Platydecticus*. A second poorly known South American decticine is *Anacanthopus capito*, described by Germain (1903: 62–63) from La Mocha Island and Angol, Chile. It is a fully winged species, said to be near *Decticus*, and was mentioned without additional information by Porter (1933: 223). According to Neave's 1939 Nomenclator Zoologicus, the generic name *Anacanthopus* is twice preoccupied.

A third South American decticine, the only one well characterized and illustrated, is *Platydecticus angustifrons*, described by Chopard (1951) from 2 males and 2 females from Cerro Chapelco, Neuquén, Argentina. A female in the U. S. National Museum was taken at Pucará, Neuquén, Jan. 1, 1960.

These 2 localities are near San Martin de los Andes. The Cerro is about 5 km. east of S. Martin, and Pucará is a peninsula on the south side of Lago Lacar about 25 km. west of S. Martin. Pucará is the site of a forestry station occupied for some years by the late Sergio Schajovskoi (deceased 1974) who made the initial collections on Cerro Chapelco. A sketch map of this area was given by Liebermann (1949: 130). Liebermann (1954: 173) has reported 2 additional specimens, which were regarded by Dr. Chopard as *P. angustifrons*, collected by Luis E. Peña G. in nearby Chile in 1948.

Key to two species of *Platydecticus*

(Multiple characters have been utilized in this key in order to compare the species in detail and make a separate diagnosis unnecessary)

Posterior margin of pronotum strongly concave; short tegmina present in male, absent in female; base of front tibia bearing a slitlike tympanal opening on each side; general dorsal color brown; size larger, length of hind femur of female about 9.0 mm; male cercus short, thick, at apex with strong mesal armed hook; male subgenital plate rather wide, feebly concave posteriorly; female ovipositor with only moderate curvature of ventral valve in apical third (Chopard, 1951, fig. 7); front femur of female proportionately stouter, ratio of length to greatest width 3.0 (Province of Neuquén, Argentina; adjacent parts of Chile) .... *angustifrons* Chopard

Posterior margin of pronotum not concave; tegmina entirely absent; tympanal openings absent; general dorsal color black, appearing green in favorable light; size smaller, length of hind femur of female about 7.5 mm; male cercus thick only at extreme base, with remainder tapering sharply and curved with 2 small toothlike spines basad of middle; male subgenital plate highly specialized, with median posterior spine (fig. 5, *mps*); ovipositor with strong curvature of ventral valve in apical third (fig. 1); front femur of female more slender, ratio of length to greatest width 4.5 (Province of Mendoza, Argentina) .... *anaesegalae*, n. sp.

Platydecticus anaesegalae, new species

Figs. 1–6

Male (holotype).—General appearance as in fig. 1 except for ovipositor. Dorsal surface microscopically roughened reticulate, with sparse minute setae. Ventral surfaces smooth. Fastigium narrow, as in *angustifrons*, conspicuously sulcate. Face and genae with widely separated surface pits. Maxillary palpi with last 3 segments subequal, apical one cylindrical, truncate. Antennae simple, segments longer than wide. Pronotal shape as in figure 2, greatest width slightly in front of middle; a trace of a median longitudinal line. Without tegmina or wings. Abdomen unspecialized except for copulatory organs. Supraanal plate small, apically rounded. Subgenital plate (fig. 5) with apical half shieldlike, expanded toward apex, apical margin with sharp upturned median spine; styli simple, clublike. Cercus conspicuously curved when seen in lateral or mesal view (fig. 4), broad at base, tapering rapidly, 2 subbasal teeth, one much the larger. Titillators, if present, not dissected due to fragility of specimens; not found in *angustifrons* by Chopard.

All 3 femora as in fig. 1, unarmed. Front tibia subcylindrical, non-sulcate, with 5 pairs of well-spaced ventral spurs, including the somewhat larger apical pair, 1 pair of dorsal apical spurs, no trace of tympanum. Mid-tibia with 5 pairs ventral spurs, 1 dorsal spur on posterior side basad of middle, and 2 small dorsal apical spurs. Hind tibia with 6 pairs dorsal spurs, the outer ones increasingly longer toward apex, inner ones

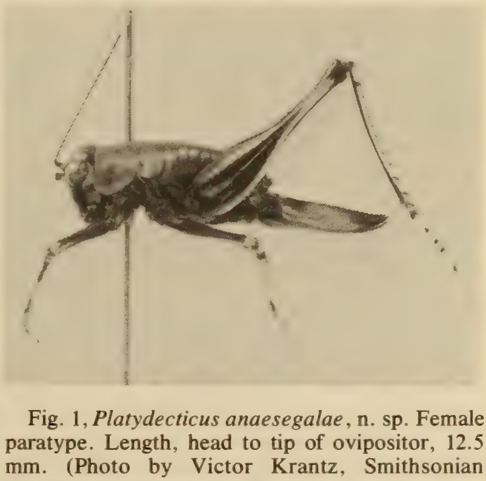
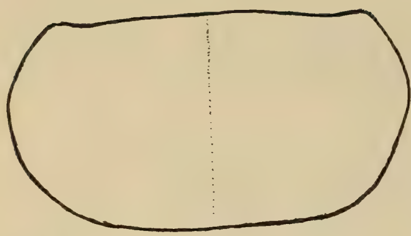


Fig. 1, *Platydecticus anaesegalae*, n. sp. Female paratype. Length, head to tip of ovipositor, 12.5 mm. (Photo by Victor Krantz, Smithsonian Photographic Laboratory).



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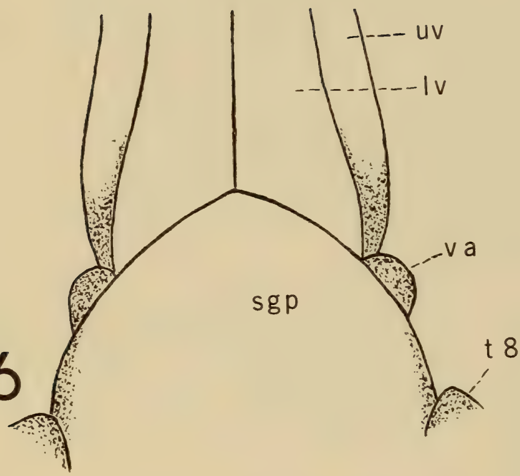

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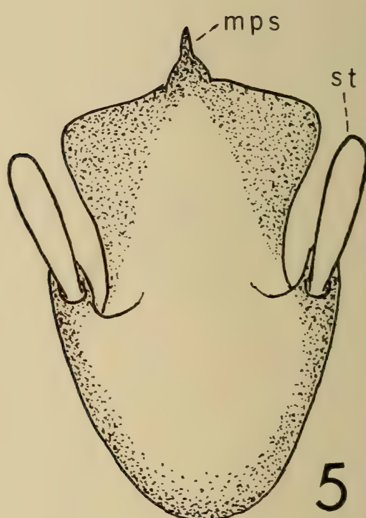

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Figs. 2–6, *Platydecticus anaesegalae*, n. sp. 2–5, male holotype; 6, female paratype. Fig. 2, dorsal view of pronotum; fig. 3, dorsolateral view of right cercus; fig. 4, mesal view of left cercus; fig. 5, ventral view of subgenital plate; fig. 6, ventral view of subgenital plate and base of ovipositor. Shaded areas dark. Abbreviations: lv, lower valve of ovipositor; msp, median posterior spine; sap, supra-anal plate; sgp, subgenital plate; st, stylus; t8, tergum 8; uv, upper valve of ovipositor; va, first valvifer. (Drawings by A. B. G.)

shorter, several minute, a pair of longer ventral apical calcars, also 1 pair smaller subapical spurs. Tarsi smooth, shiny, unarmed; length proportions of hind tarsomeres as 16: 12: 6: 21: plantulae of basal hind tarsomere short, inconspicuous; short divided pulvilli on tarsomeres 1–3; claws simple, equal; no arolium.

Female (fig. 1).—Essentially as in male except for larger size and genital structures. Subgenital plate in ventral view (fig. 6) entire, bluntly obtuse at posterior margin. Ventral valve of ovipositor strongly armed with serrations on and near ventral margin, extending less than half distance to base; dorsal valve armed on and near dorsal margin, but serrations extending fully half distance to base.

Coloration.—Most of dorsal surface of head, thorax, and abdomen blackish; in strong natural light and in some artificial light showing iridescent green; fastigium dull orange; eyes brownish; ventral surface tan; face dark brown, clypeus and labrum pale; antennae black except for pale ventral surface of segment 1. Legs marked as in fig. 6; hind femur black except ventrally and

ventral part of outer face, including more pale toward apex, knee dark.

Measurements in mm (male holotype and 2 female paratypes).—Body length, male 6.7, female 12.5–13.0; pronotum length, male 1.5, female 1.9–2.0, pronotum width, male 2.9, female 3.2; front femur, male 2.1, female 2.6; hind femur length, male 6.3, female 7.5–7.8; hind femur width, male 1.5, female 1.9–2.1; hind tibia, male 4.8, female 5.7; ovipositor, female 4.5.

Specimens examined.—13 (3 males, 5 females, 5 nymphs).

Type locality.—ARGENTINA, Department of Las Heras, Province of Mendoza, west watershed of Cerro Aconcagua (32° 30′ S, 70° 03′ W.) 4,250 m. (13,944 ft.). Holotype ♂, allotype ♀, 2 ♂♂, 2 ♀♀, 5 nymphs, 16 Feb. 1973. Near Cerro Tolosa (of Cerro Aconcagua region), about 4,300 m, Feb. 1974, 2 ♀♀. All collected by J. M. Baron and F. Gratton. Holotype and paratypes in U. S. National Museum of Natural History, Washington, D. C. (USNM Type 73352); allotype and paratypes in Museo Argentino de Ciencias Naturales, Buenos Aires, Arg.; paratypes in Museo de La Plata, Arg.

The specific name is chosen as a tribute to Sra. José Liebermann (née Ana Ethel Segal) who as a devoted helpmate to her husband throughout his long career so richly merits the association with this tiny but distinctive denizen of lofty places.

It is quite possible that when thorough analyses of South American genera are made, especially as additional species are found, the 2 species now assigned to *Platydecticus* will warrant generic separation. Of the characters distinguishing the 2 species, the presence or absence of tegmina and auditory tympana, and the concave vs. straight posterior margin of the pronotum may be most fundamental.

Sr. Baron has supplied us with a detailed account of the climb on Aconcagua and the circumstances of the Feb. 16, 1973 capture. He and Dr. Gratton departed on foot from Puente del Inca, Argentina (2,752 m.) on Feb. 12, by way of the Rio Horcones Valley, passing through Confluencia (3,100 m.). The route led through the section known as the "gran playón" ("grand beach"), a desertlike terrain with alluvial and diluvial moraines. The trail continued up talus slopes amid conditions of extreme cold and winds above 100 km/hr. The collection was made Feb. 16 after spending the night in subzero centigrade conditions at a refuge shelter named General of the Division Juan Carlos Sánchez.

The decticines were collected between 10 and 11:30 a.m., starting before there was warmth from the morning sun. They were in an area of rocky talus debris, with small stones and large bare rocks as well as some ice. Small lichens occurred, but no higher plants. The woody plants had ended in this area at 3,800 m. A composite, *Chaetanthera pulvinata* (Philippi) Hauman, occurs at about 4,000 m., also some Umbelliferae, and higher up one of the Bignoniaceae, *Argylia uspallatensis* DC. The decticines were difficult to catch because of their vigorous jumping and cryptic coloration. Adults jumped easily, and in any direction, in arcs up to 50 cm high and 120 cm long (20 × 47 inches). Sr. Baron mentioned the brilliant green trimming on the back of some active specimens.

The altitude of the type locality of *P. anaesegalae* is considerably more than half the altitude of the summit of Cerro Aconcagua, 6,929 m. (22,834 ft.), though less than half the altitude to the summit from the start of the climb at Puente del Inca. FitzGerald (1899) discussed the Aconcagua region, with numerous photographs of the terrain. It appears that on FitzGerald's map the type locality is somewhat north and directly east of Los Dedos (The Fingers).

In February 1974 Baron and Gratton made another trip and collected 2 females near Cerro Tolosa, a secondary peak near Aconcagua.

The occurrence of *Platydecticus anaesegalae* at high elevations near the border of Argentina and Chile is a re-

minder that some of the notable records of high-altitude Orthoptera are based on Decticinae. *Hyphinomos fasciata* Uvarov, a decticine from 4,575–4,880 m. (15,000–16,000 ft.) in western Tibet, was reported to represent the highest record for Orthoptera (Uvarov, 1921: 75). However, the current record, 5,490 m. (18,007 ft.) is attributed by Mani (1968: 103) to unidentified nymphs of a typical grasshopper (Acrididae) from near Mt. Everest. Another acridid from the Mt. Everest area, and likewise collected by the noted explorer Major R. W. G. Hingston, was described by Uvarov (1925: 171) as *Dysanema malloryi* from 4,875 m. (16,000 ft.), but (*l.c.*: 165) he evidently regarded the unidentified nymphs as a different grasshopper.

Rehn and Hebard (1920: 258–263) reported a then new species of decticine, *Acrodectes philopagus*, from 14,500 ft. (4,420 m.) at the summit of Mt. Whitney, California, highest mountain in the continental United States except Alaska. Tinkham (1944: 274–277, figs. 5, 6) discussed the habitat of *philopagus* at 12,000 to 13,000 ft. on Mt. Whitney, and it is interesting, in view of the green color of *Platydecticus anaesegalae* in certain light, that he found the body color of living specimens of *philopagus* "a beautiful mottled green with flecks of black everywhere on the abdomen." The few museum specimens of *philopagus* that we have seen are various shades of brown and black. No mention of green color was made by Rehn and Hebard, or by Rentz and Birchim (1968: 126), who gave further biological notes.

What is certainly the most widely known species of Decticinae in the United States, *Anabrus simplex* Haldeman, the Mormon cricket, also is noteworthy for tolerance of high altitudes. Although it is best known for range and crop injury at moderate elevations in numerous western states, Alexander and Hilliard (1969: 415–416) referred to it as widespread and able to develop at varying altitudes, going in numbers onto the tundra of Colorado, even above 13,000 ft.

Lastly, we note that Mani (1968: table 20, p. 136) gave the highest record for Tettigoniidae in the South American Andes as 4,900 m.; however, we have not found a determination of the species to which he referred.

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Parasitic Hymenoptera Associated with Bruchid-Infested Fruits in Costa Rica

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ABSTRACT

Some 43 species of parasitic wasps were reared from bruchid-infested fruits of various legumes and certain other plant families in Costa Rica, and represent 1 bethylid and several braconid and chalcidoid genera. These species are discussed briefly in an annotated list, and simple keys intended for field use are provided for the braconids and chalcidoids. Species thought to be primary larval bruchid parasites belong to the braconid genera *Allorhogas* (1), *Heterospilus* (5), *Percnobracon* (?, 1), *Stenocorse* (1), and *Urosigalphus* (2), and to the chalcidoid genera *Chryseida* (1), *Eupelmus* (3), *Horismenus* (3), *Spilochalcis* (5), and *Torymus* (?, 1). Of these, the *Spilochalcis* seem to be specialists on *Amblycerus*, each host species having apparently a different parasite, and the *Torymus* might be specific on *Zabrotes*; these 2 bruchid genera are Amblycerinae. The other wasp genera are generalists, apparently attacking various genera of Bruchinae; some *Eupelmus, Horismenus,* and *Urosigalphus* attack members of both *Amblycerus* and Bruchinae, though especially among the *Urosigalphus* the wasp "species" may be sibling complexes. Field work is needed to clarify host-parasite relationships, host-induced variability, and several other problems described in this paper.

Bruchid beetles form one of the prominent groups of seed predators reared from dry fruits in arid tropical lowlands. A number of systematists and ecologists, notably D. H. Janzen and colleagues, are studying bruchid-plant interactions and frequently encounter parasitic wasps in association with reared bruchids; for a preliminary account of ecological implications concerning these parasites, see Janzen (1975: 177–181). Janzen has obtained large numbers of reared bruchid samples principally from Guanacaste Province, Costa Rica, and from these I have accumulated a considerable body of parasite data. Here, I provide an annotated list of these parasites, along with simple keys to the braconids and chalcidoids based on determinations by P. M. Marsh and G. Gordh, respectively. The purposes are to facilitate field identifications, to provide a basis for comparisons with parasite faunas elsewhere in tropical America, to indicate which of the various wasps are plausible bruchid parasites as opposed to moth or seed parasites, to comment on levels of systemic knowledge, and to indicate associations that might be suitable for detailed comparisons.

Collections were made by gathering ripe fruit crops, sealing them in plastic bags, and awaiting insect emergence over a several-month period. Consequently, there is no direct indication of parasitism. Generally, all beetles and all parasites were kept, but moths were generally discarded and, frequently, no records kept of their presence. This means that interpretation of actual hosts is provisional, and, if 2 or more bruchid species occurred in a given sample, then there is no way to distinguish the actual host. Detailed studies are needed to determine actual parasitism; many of the parasite species or species complexes probably cannot be worked out systematically until large samples are obtained from controlled rearings. Some of the putative bruchid parasites appear to be generalists, attacking numerous genera or even spanning subfamilies; others appear to be specialists, restricted to

single genera or even species. In consequence, I attempt here to recognize such situations in order to suggest particular systems that might be relatively simple to analyze taxonomically and which might readily yield interesting comparisons.

I must emphasize here that, at species level, this treatment is highly provisional on 2 counts: the collections are preliminary and hence the species representation may be far from complete, though I think that all of the most commonly encountered forms are represented; and the systematics, and species level discrimination, particularly for the chalcidoids, is unsettled. I take full responsibility for discrimination among some of the chalcidoids, as Gordh's analysis is not in full agreement with mine: notably, I distinguish several forms of *Spilochalcis* not distinguished by him, and I distinguish fewer forms of *Horismenus* and *Eurytoma* than he did. These differences of opinion are discussed as appropriate. Generally, I give summary statements for each species, but in some instances specific samples are cited by code number: these voucher numbers form an index to my records, Janzen's records, and mounted specimens from the reared materials which are deposited in the National Museum of Natural History, Washington, D. C. (USNM).

Family Bethylidae

Genus ***Parasierola*** Cameron

Parasierola sp.—1 specimen, sample #1974–2, *Acacia "riparia"* complex, bruchids *Stator vittatithorax* (Pic) and *S. limbatus* (Horn). According to A. S. Menke (pers. comm.), members of this genus are known mostly from Microlepidoptera but some have been recorded from bruchids. Bridwell (1919b) lab-tested them successfully on *"Caryoborus monandra"* (= *Caryedon serratus* (Olivier), J. M. Kingsolver pers. comm.) in Hawaii. Therefore, this probably is an incidental parasite of *Stator* spp., but its apparent low incidence suggests that ecological studies would not be rewarding.

Family Braconidae

1. Abdomen rigid and carapace-like, formed by fusion of terga 1–3 2
   Abdomen not carapace-like, terga separated by distinct sutures 4
2. Outer claw of hind tarsus not larger than inner claw .... *Chelonus (Microchelonus)* sp.
   Outer claw of hind tarsus larger than inner claw 3
3. Large, over 3.5 mm .... *Urosigalphus (Bruchiurosigalphus) panamaensis*
   Small, under 3.5 mm .... *Urosigalphus (Bruchiurosigalphus) aquilus*
4. Wing venation reduced; face without an opening between clypeus and mandibles .... *Apanteles* spp.
   Wing venation not reduced; face with a circular opening between clypeus and mandibles 5
5. Wings banded .... *Percnobracon* sp.
   Wings not banded 6
6. Occipital carinae absent .... *Bracon* spp.
   Occipital carinae present 7
7. First intercubital vein of forewing present, 1st and 2nd cubital cells separated 8
   First intercubital vein absent or weak, 1st and 2nd cubital cells not separated 9
8. Posterior terga coarsely microsculptured; 1st segment of mediella of hindwing equal in length to 2nd .... *Stenocorse bruchivora*
   Posterior terga polished or finely microsculptured; 1st segment of mediella shorter than 2nd .... *Allorhogas* sp.
9. Head punctate behind ocelli .... *Heterospilus* sp. #1
   Head transversely striate behind ocelli 10
10. Thorax coarsely punctate .... *Heterospilus* sp. #2
    Thorax not coarsely punctate except along notaulices 11

11. Postocellar striations fine, weak .... *Heterospilus* sp. #3
Postocellar striations strongly developed 12

12. Body dark .... *Heterospilus* sp. #4
Body pale .... *Heterospilus* sp. #5

Genus ***Allorhogas*** Gahan

Allorhogas sp.—3 samples from *Lysiloma* and 1 from *Albizzia*; bruchids *Merobruchus* spp. in *Albizzia* sample and 2 *Lysiloma* samples, *Stator limbatus* (Horn) in *Albizzia* sample. According to Marsh (pers. comm.) this may be a bruchid parasite; if so, I suspect it is specialist on *Merobruchus*. Studied in conjunction with the species of *Heterospilus* and *Stenocorse*, ecological comparisons should prove useful.

Genus ***Apanteles*** Foerster

These are moth parasites exclusively.

Genus ***Bracon*** Fabricius

These probably are moth parasites and thus merit only brief comment here. One species is frequent in samples from legumes including *Acacia, Bauhinia, Cassia,* and *Lysiloma,* with various genera of Bruchinae; another was found in 1 sample of *Guazuma* (#17-4), with a mixture of *Amblycerus* (Amblycerinae), *Acanthoscelides* (Bruchinae), *Cymatodera* (Cleridae), and *Tricornynus* (Anobiidae); and a third was found in 1 sample of *Ipomoea* (#20-27), with *Megacerus* sp. (Bruchinae).

Genus ***Chelonus*** Panzer, subgenus ***Microchelonus*** Szepligeti

Chelonus sp.—1 specimen, sample #20-42, *Cassia biflora* L., bruchids *Sennius* spp. This is a moth parasite.

Genus ***Heterospilus*** Haliday

These probably are bruchid parasites, and are detailed below. The species are apparently readily distinguished, and will be taxonomically revised in the near future by Marsh. This group needs intensive field investigation; I suspect that numerous additional species will be found associated with bruchid-infested fruits. Some species may be specialists, but preliminary indications are that they more likely are generalists at subfamily level on Bruchinae. I suspect that interesting comparisons may show up between the species, and among the related genera *Allorhogas, Heterospilus*, and *Stenocorse*.

Heterospilus sp. #1.—1 sample (#20–35), unidentified mimosaceous shrub, bruchid genera *Acanthoscelides* and *Stator*.

Heterospilus sp. #2.—5 samples, legume genera *Acacia, Cassia,* and *Mimosa*, bruchid genera *Acanthoscelides, Merobruchus,* and *Sennius*.

Heterospilus sp. #3.—4 samples, legume genera *Acacia, Albizzia,* and *Bauhinia*, bruchid genera *Caryedes, Gibbobruchus, Merobruchus,* and *Stator*.

Heterospilus sp. #4.—1 sample (#72-016), *Bauhinia glabra* Jacq., bruchids *Caryedes cavatus* Kingsolver and Whitehead and *C. x-liturus* (Pic).

Heterospilus sp. #5.—1 sample (#72-005), host plant not known and no bruchids reared.

Genus ***Percnobracon*** Kieffer

Percnobracon sp.—3 specimens from 2 separate rearings from the same tree in 1972 (#20-43) and 1975 (#6-75-16). Janzen (pers. comm.) states that this tree may be either an *Albizzia* or a *Lysiloma*; judging from examination of leaves and fruits, I suspect it is related to *A. caribaea* (Urb.) Britt. & Rose. Marsh (pers. comm.) suspects that this wasp is a beetle parasite, and bruchids from these samples include *Merobruchus* spp. and *Stator limbatus* (Horn). I suspect that, if it is indeed a bruchid parasite, it specializes on *Merobruchus* spp. which seem to have an extraordinarily broad range of parasites and frequently a heavy parasite load. Despite the apparent low incidence, ecological investigation is desirable because parasites in fruits of *Albizzia* and *Lysiloma* in general are especially rich in numbers and diversity.

Genus ***Stenocorse*** Marsh

Stenocorse bruchivora (Crawford).—This is by far the most abundant braconid found in bruchid-infested fruits in Costa Rica, and probably is parasitic on all species of bruchinae. There is, however, a great range of variation in size and color, and detailed study may indicate correlation with particular hosts. In particular, I suspect that parasites of *Megacerus* may differ from those of other Bruchinae because of the peculiar biological and morphological features of *Megacerus*.

Genus ***Urosigalphus*** Ashmead, subgenus ***Bruchiurosigalphus*** Gibson:

These are bruchid parasites, and are detailed below. This genus was recently reviewed by Gibson (1972), but the species are difficult to distinguish and his keys difficult to use, and the neotropical fauna at the time of the revision was poorly sampled; I report here Marsh's conclusions, but these differed from mine. However, these wasps as a group are easy to distinguish and, moreover, I suspect that detailed ecological investigations will be rewarding for both ecologist

and systematist. Both species listed here are apparently generalists, from host species spanning two bruchid subfamilies, but I suspect that sibling complexes may be involved especially in the case of *U. aquilus*.

Urosigalphus aquilus Gibson.—2 samples from *Cordia alliodora* (Ruiz & Pav.) Cham. (Boraginaceae), with bruchids *Amblycerus* sp. (Amblycerinae); and 2 samples from *Lysiloma* sp., with bruchids *Merobruchus* sp. in both and *Stator limbatus* (Horn) in one. Although Marsh determined all as *U. aquilus*, I originally had the *Cordia* and *Lysiloma* samples distinguished as different species. I suspect that this parasite represents a sibling complex.

Urosigalphus panamaensis Gibson.—several samples from various Leguminosae *s. l.*, with bruchid genera *Merobruchus, Mimosestes*, and *Stator*; and 1 sample from *Cordia gerascanthos* Jacq. (Boraginaceae), with bruchid *Amblycerus* sp. The latter (#20-14) again suggests that a sibling complex may be involved though in this instance I did not have any impression from morphological examination that this wasp might be distinct.

Superfamily Chalcidoidea

1. Body mostly yellow 2
   Body mostly dark 9
2. Hind femur swollen 3
   Hind femur not swollen 7
3. Scutellum not distinctly vittate 4
   Scutellum distinctly vittate 5
4. Prescutum not distinctly vittate; under 4 mm .... *Spilochalcis* sp. #1
   Prescutum distinctly vittate; over 4 mm .... *Spilochalcis* sp. #2
5. Parapsida with distinct spots .... *Spilochalcis* sp. #5
   Parapsida without distinct spots 6
6. Under 5½ mm .... *Spilochalcis* sp. #3
   Over 5½ mm .... *Spilochalcis* sp. #4
7. Petiole short, inconspicuous; forewing not stigmated ....
   .... Encyrtidae, ?genus sp. #1
   Petiole long; forewing conspicuously stigmated 8
8. Petiole black .... *Eudecatoma* sp., males
   Petiole yellow .... *Eudecatoma* sp., females
9. Pronotum with fine median longitudinal sulcus 10
   Pronotum without median sulcus 12
10. Pronotum with microsculpture granulose .... *Horismenus* sp. #1
    Pronotum with microsculpture flattened 11
11. Median carina of propodeum wide .... *Horismenus* sp. #2
    Median carina of propodeum narrow .... *Horismenus* sp. #3
12. Head and thorax coarsely sculptured, punctate 13
    Head and thorax not coarsely punctate 18
13. Wings pictured .... Eurytomidae, ?genus sp.
    Wings not pictured 14
14. Head and thorax distinctly metallic blue or green; interantennal process ..... long, apex acute .... *Chryseida* sp. cf. *bennetti*
    Head and thorax black; interantennal process short, apex not acute 15
15. Neck with dull, granulose microsculpture; female gaster bicolored, microsculptured above .... "*Eurytoma*" sp. #1
    Neck with shiny, flattened microscultpure; female gaster black, polished above 16
16. Punctation of head and parapsides feeble .... *Bruchophagus* sp.
    Punctation of head and parapsides strong 17

17. Coxae and tegulae yellow .... "*Eurytoma*" sp. #2
Coxae and tegulae black .... "*Eurytoma*" sp. #3

18. Mesopleuron deeply grooved to receive femur 19
Mesopleuron not deeply grooved to receive femur 26

19. Hind coxa enlarged; ovipositor long .... *Torymus* sp.
Hind coxa normal; ovipositor short 20

20. Hind tibia with 1 apical spur; head striate above mouth 21
Hind tibia with 2 spurs; head not striate above mouth 24

21. Microsculpture of notum flattened .... Pteromalidae, ?genus sp. #1
Microsculpture of notum granulose 22

22. Microsculpture of face flattened .... Pteromalidae, ?genus sp. #2
Microsculpture of face granulose 23

23. Head and pronotum with greenish reflections .... Pteromalidae, ?genus sp. #3
Head and pronotum with purplish reflections .... Pteromalidae, ?genus sp. #4

24. Legs wholly yellow .... Encyrtidae, ?genus sp. #2
At least hind tibia infuscated 25

25. Hind femur infuscated .... *Eupelmus* sp. nr. *cushmani*, males
Hind femur yellow .... *Eupelmus* sp. nr. *peruvianus*, males

26. Apical 4/5 of wing infuscated .... Eupelmidae, ?genus sp.
Wing hyaline 27

27. Frontal sulci not carinate externally; scape metallic; ovipositor short, not annulate .... *Eupelmus* sp. nr. *peruvianus*
Frontal sulci carinate externally; scape yellow or metallic; ovipositor long, annulate 28

28. Scape more or less metallic; hind tibia partly infuscated .... *Eupelmus* sp. nr. *cyaniceps*
Scape yellow; hind tibia not or slightly infuscated .... *Eupelmus* sp. nr. *cushmani*

Family Chalcididae

Genus ***Spilochalcis*** Thomson

These probably are bruchid parasites specializing on *Amblycerus* (subfamily Amblycerinae), and are detailed below. This is a very large genus parasitic principally on Microlepidoptera, but the bruchid parasites apparently form a discrete group characterized by tridentate mandibles (Gordh pers. comm.) and therefore should be relatively simple taxonomically. Probably, none of the Costa Rican species are described. Gordh determined these as a single, variable species, but my impression is that each *Amblycerus* has a different parasite: the differences are minute and concern size and pigmentation, but specimens from each series are constant and series from different rearings from the same host species are uniform. From the standpoint of specificity, this seems to be the most desirable bruchid-parasite system to study. Also, *Spilochalcis* appears to be the only parasite associated exlusively with any single genus, and the only one to be associated exclusively with Amblycerinae. I anticipate that the systematics of *Amblycerus* will be worked out in the near future, and that the systematics of pertinent *Spilochalcis* should be possible to work out readily if required. At the outset, however, it will be necessary to determine if there is a sibling complex, or if the observed variation is host induced.

Spilochalcis sp. #1.—1 sample (#20-8), *Cordia alliodora* (Ruiz & Pav.) Cham., bruchid *Amblycerus* sp.

Spilochalcis sp. #2.—1 sample (#17-6), *Guazuma ulmifolia* Lam., bruchids *Amblycerus cistellinus* (Gyllenhal) and *Acanthoscelides guazumicola* Johnson and Kingsolver.

Spilochalcis sp. #3.—1 sample (#19-12), *Combretum farinosum* H.B.K., bruchid *Amblycerus perfectus* (Sharp).

Spilochalcis sp. #4.—1 sample (#20-1), *Cassia emarginata* L., bruchid *Amblycerus* sp.

Spilochalcis sp. #5.—2 samples (#1972-022, #1974-48), *Cassia obtusifolia* L., bruchid *Amblycerus* sp. The other *Spilochalcis* samples are small, but these are sufficiently well represented and uniform to indicate that differences among the *Spilochalcis* "species" are not due to random variation.

Family Encyrtidae

?Genus sp. #1

One sample (#6-75-16), *Albizzia* or *Lysiloma*, bruchids *Acanthoscelides* sp. (possible contaminant) and *Merobruchus* spp. I suspect this is a hyperparasite.

?Genus sp. #2

One fragmentary specimen (#20-49), *Cassia skinneri* Benth., bruchid *Acanthoscelides obrienorum* Johnson. This probably is a hyperparasite.

Family Eulophidae

Genus ***Horismenus*** Walker

These are bruchid parasites, and are detailed below. Nearctic species were recently revised by Burks (1971a) but the Neotropical species remain problematic. Gordh sorted out more forms than I distinguish here, and I comment on these as appropriate. I suspect that until the systematics of these bruchid parasites are worked out there can be no useful ecological comparisons made, but detailed rearings will make systematic analysis possible. The "species," in the sense used here, apparently parasitize all members of Amblycerinae and Bruchinae.

Horismenus sp. #1, cf. ***missouriensis*** (Ashmead).—This is the most frequently reared and most abundant of the chalcidoid parasites, reared from fruit crops of various Leguminosae *s.l.*, *Guazuma ulmifolia* Lam., *Ipomoea* sp., and *Cordia gerascanthos* Jacq.; most of these samples contained representatives of Bruchinae, but that from the *Cordia* (#20-14) contained *Amblycerus* only. One sample (#1972-016, *Bauhinia glabra* Jacq., bruchids *Caryedes cavatus* Kingsolver and Whitehead and *C. x-liturus* (Pic)) had a single *Horismenus*, regarded by Gordh as probably a different species because of strongly cupreous coloration.

Horismenus sp. #2.—1 specimen (#19-17). This sample was of *Phaseolus lunatus* L. but was contaminated with fruits of *Cordia alliodora* (Ruiz & Pav.) Cham.; bruchids from the *Phaseolus* were *Acanthoscelides argillaceus* (Sharp) and *Zabrotes subfasciatus* (Boheman), and the bruchid from the *Cordia* was *Amblycerus* sp. Our only record of *Torymus* sp. is also from this sample. Possibly, the peculiar *Horismenus* and *Torymus* records are associated with the *Zabrotes* (Amblycerinae), as this is the only reared *Zabrotes* sample with associated parasites; it also is possible that this *Horismenus* is not a bruchid parasite.

Horismenus sp. #3.—3 samples. Sample #17-4, *Guazuma ulmifolia* Lam., with bruchids *Amblycerus cistellinus* (Gyllenhal) and *Acanthoscelides gauzumicola* Johnson and Kingsolver, included *Horismenus* sp. #1 as well. Sample #19-23, unidentified Mimosaceae, bruchids *Stator limbatus* (Horn), *S. vittatithorax* (Pic), and *Merobruchus* sp., had 9 specimens; Gordh sorted out 1 specimen each of 2 extra species based on differences in color of the antennal scape, but I think these differences reflect infrapopulational variation. Sample #20-2, *Acacia farnesiana* (L.) Willd., bruchids *Mimosestes* sp., apparently had the same *Horismenus* but these were not checked by Gordh. I suspect that this is a bruchid parasite, but as noted above there may be a complex of species grouped here.

Family Eupelmidae

?Genus sp.

One specimen (#20-10), *Piscidia carthagenensis* Jacq., bruchids *Ctenocolum crotonae* (Fahraeus) and *C. janzeni* Kingsolver and Whitehead. According to Gordh (pers. comm.) this resembles forms that normally parasitize eggs of Orthoptera, and hence probably is not a bruchid parasite.

Genus ***Eupelmus*** Dalman

These are bruchid parasites, and are detailed below. This genus is very large, but the bruchid parasites apparently are few and should be readily distinguishable given sufficient study; nomenclatural problems, however, may be difficult. The bruchid parasites belong to 2 distinct groups, with *E.* cf. *peruvianus* in one group and the other forms in the other. I distinguished 3 female forms but only 2 male forms; association is tentative, but all samples with male "*peruvianus*" also had females, and most samples with male "*cushmani/cyaniceps*" also had females. The wasps are generalists, the group represented by "*peruvianus*" attacking both Amblycerinae and Bruchinae and the group represented by "*cushmani/cyaniceps*" attacking at least various Bruchinae.

Eupelmus sp. nr. ***peruvianus*** (Crawford).—Numerous samples, bruchids of subfamilies Amblycerinae and Bruchinae.

Eupelmus sp. nr. ***cushmani*** (Crawford).—Numerous samples, bruchids of various genera of Bruchinae.

Eupelmus sp. nr. ***cyaniceps*** Ashmead.—5 specimens from 3 samples, only. This form is variable in coloration, and perhaps is just a variant of "*cushmani*". More investigation is needed to determine taxonomic status and to determine if this is a bruchid parasite. Samples included various Bruchinae.

Family Eurytomidae

?Genus sp.

One fragmentary specimen (#6-75-16), *Albizzia* or *Lysiloma*, bruchids *Merobruchus* sp., *Stator limbatus* (Horn), and *Acanthoscelides* sp. (con-

taminant?). More investigation is needed to determine what this is both taxonomically and ecologically; it evidently is not an abundant bruchid parasite, if indeed a bruchid parasite at all.

Genus ***Bruchophagus*** Ashmead

Bruchophagus sp.—3 samples from *Indigofera* and 1 specimen from *Cordia alliodora* (Ruiz & Pav.) Cham. (#20-8). The *Indigofera* samples contained the bruchid *Acanthoscelides kingsolveri* Johnson; the *Cordia* sample contained *Amblycerus* sp. I suspect there are 2 forms confused here, but my original sortation was confirmed by Gordh. As I am not aware of any other parasite that specializes on particular species of Bruchinae, I suspect that the *Indigofera* form is a seed chalcid rather than a bruchid parasite.

Genus ***Chryseida*** Spinola

Chryseida sp. nr. ***bennetti*** Burks—numerous samples from several plant families, bruchids of various genera of Bruchinae. This undoubtedly is a bruchid parasite, generalist at least on Bruchinae. These specimens are highly variable and Gordh thought that there might be more than 1 species, but his identifications split series and I therefore suspect that only 1 species is involved. The genus *Chryseida* is moderately large, but the bruchid parasites apparently are easily distinguished; it is necessary only to determine that there is only 1 species, and that it is conspecific with the Texan *bennetti*, to open the door to useful ecological comparisons made over a wide geographic area.

Genus ***Eudecatoma*** Ashmead

Eudecatoma sp.—Several samples from *Albizzia* and/or *Lysiloma* contained 1 or 2 specimens each, along with bruchids of the genera *Merobruchus* and *Stator*. There is a series in USNM of the same or a related species from Barro Colorado Island, Canal Zone, labeled "*Inga* legume"; since, as far as I know, there are no bruchids associated with *Inga*, this *Eudecatoma* is probably a seed chalcid.

Genus ***Eurytoma*** Illiger

I distinguish 3 "species" as detailed below, but Gordh thought there might be others which I mention without additional comment. Some, at least, do not exactly fit the generic diagnosis given by Burks (1971), and hence are treated as "*Eurytoma*" complex. For the present, I would regard these as impossible to deal with taxonomically; most samples are poorly represented in numbers, so ecological studies are unlikely to be rewarding; and I am unable to predict which, if any, are bruchid parasites. I suspect that all are seed chalcids, from comparison of the host fruit samples among the genera *Bruchophagus, Eudecatoma,* and *Eurytoma*: all such samples so far examined are from *Cordia alliodora* (Ruiz & Pav.) Cham. (Boraginaceae) and the legumes *Albizzia/Lysiloma* and *Indigofera*.

"Eurytoma" sp. #1.—1 sample from *Indigofera* (#20-7), bruchid *Acanthoscelides kingsolveri* Johnson; and 3 samples from "*Lysiloma*", bruchid *Merobruchus* sp. Gordh distinguished 2 forms in the *Indigofera* sample, 1 of them the same as the *Lysiloma* sample.

"Eurytoma" sp. #2.—3 samples from "*Lysiloma*", bruchid *Merobruchus* sp. This may be only a variation of *E.* sp. #1; I have an impression that the *Eurytoma* from *Albizzia/Lysiloma* are variable, but because there are few good, clean specimens I cannot reach a definite conclusion.

"Eurytoma" sp. #3.—1 sample from *Cordia alliodora* (Ruiz & Pav.) Cham. (#19-7) with bruchid *Amblycerus* sp., and 3 from *Albizzia/Lysiloma* with bruchids *Merobruchus* spp. and *Stator limbatus* (Horn). Gordh distinguished several forms, tentatively, and I suspect that at least the *Cordia* form is distinct.

Family Pteromalidae

Gordh was unable to identify any of these to genus, and I suspect that their systematics currently is at a stage that would make useful ecological studies with them impossible. Some of the forms listed here are unlikely to be bruchid parasites. The cosmopolitan bruchid parasite *Choetospila elegans* Westwood is not represented in these samples.

?Genus sp. #1

One specimen (#20-7), *Indigofera*, bruchid *Acanthoscelides kingsolveri* Johnson. Probably not a bruchid parasite.

?Genus sp. #2

One specimen (#20-40), *Mimosa*, bruchid *Acanthoscelides* sp. Probably not a bruchid parasite.

?Genus sp. #3

Five samples, 4 from various legumes with bruchids of genera *Acanthoscelides, Merobruchus, Mimosestes, Sennius,* and *Stator*, and 1 (#20-13) from *Triumfetta lappula* L. (Tiliaceae) with bruchid *Acanthoscelides* sp. The associated data with these samples imply that this probably is a bruchid parasite. Gordh thinks I may have several forms confused here.

?Genus sp. #4

One large sample (#19-11), *Lysiloma*, with bruchids *Merobruchus* sp. and *Stator limbatus* (Horn). This might be a bruchid parasite, but I suspect otherwise since this sample was also rich in *Tricorynus* spp. (Anobiidae) and several species of wasps.

Family Torymidae

Genus *Torymus* Dalman

Torymus sp.—1 sample (#19-17). See discussion of *Horismenus* sp. #2. If this is a bruchid parasite, then I suspect it specializes on *Zabrotes*.

Discussion

Most of the arid-land fruit crops involved in this study are from woody plants, and most are from legumes of the families Caesalpiniaceae, Fabaceae, and Mimosaceae. Other samples containing parasitic wasps were from Boraginaceae (*Cordia* spp.), Combretaceae (*Combretum farinosum* H.B.K.), Convolvulaceae (*Ipomoea* spp.), Sterculiaceae (*Guazuma ulmifolia* Lam.), and Tiliaceae (*Triumfetta lappula* L.). Plants of many other non-leguminous families are hosts especially for *Amblycerus* species, and need to be examined carefully for parasitic wasps.

The following wasp genera are known or suspected to include common bruchid parasites in Costa Rica: 1, the related braconid genera *Allorhogas, Heterospilus*, and *Stenocorse*, on Bruchinae; 2, the braconid genus *Urosigalphus*, on Amblycerinae and Bruchinae; 3, the chalcidid genus *Spilochalcis*, on *Amblycerus*; 4, the eulophid genus *Horismenus*, on Amblycerinae and Bruchinae; 5, the eupelmid genus *Eupelmus*, on Amblycerinae and Bruchinae; and 6, the eurytomid genus *Chryseida*, on Bruchinae. The following genera have species which might be bruchid parasites and, if so, are for various reasons worthy of investigation: 7, the braconid genus *Percnobracon*, on Bruchinae (?); and 8, the torymid genus *Torymus,* on *Zabrotes* (?). These are potentially important larval parasites; no egg parasites are reported here. All other genera reported herein either probably do not include primary bruchid parasites in Costa Rica, or appear to be of too low abundance to permit useful bruchid-parasite comparisons. The taxonomy of the various parasite groups is of various complexity and at various levels of knowledge. The braconids seem comparatively simple, though there may be species-level problems in *Stenocorse* and *Urosigalphus*, and necessary taxonomic work can probably be accomplished readily. The chalcidoids are much more poorly known and hence more problematic, but with the possible exception of *Eupelmus* I expect that these problems can be resolved readily.

The bruchids involved are members of the subfamilies Amblycerinae (*Amblycerus* and *Zabrotes*) and Bruchinae (numerous genera). Parasites associated with *Amblycerus* include some members of *Urosigalphus, Spilochalcis, Horismenus*, and *Eupelmus*; of these, the *Spilochalcis* are known only from *Amblycerus*, and each species may be host specific; and I suspect also that the *Urosigalphus* of *Amblycerus* may differ from those of Bruchinae. No parasites are definitely associated with *Zabrotes*, but a species of *Torymus* may be. The parasites of Bruchinae include all of the above list except *Spilochalcis* and *Torymus*; no specificity is apparent, but parasites of *Megacerus* are likely to differ from those of other Bruchinae. Members of some Bruchinae genera, notably *Merobruchus*, tend to have extraordinarily heavy parasite loads both in numbers and diversity; thus, their host plants, especially members of the mimosaceous genera *Albizzia* and *Lysiloma*, merit particular attention. Studies of most of the major groups of Central American Bruchidae have been completed or are currently in progress. The principal group still awaiting attention is *Acanthoscelides*, and it is anticipated that even this group will have been studied within the next few years. Thus, there soon should be no major problems with bruchid systematics to interfere with ecological studies.

I expect that the general pattern outlined for Costa Rica will apply to tropical America generally, but not to other parts of the world. Bridwell (1918, 1919a, 1920), in studies of the parasites of the introduced Hawaiian

bruchids, reported the following: 1, the trichogrammatid egg parasite *Uscana semifumipennis* Girault, described from Texas and probably occurring in Costa Rica; 2, the American braconid *Heterospilus prosopidis* Viereck—probably the same as 1 of the *Heterospilus* reported herein; 3, the bethylid *Scleroderma immigrans* Bridwell, probably of Asiatic origin; 4, the endemic eupelmid *Charitopodinus swezeyi* (Crawford), an apparently incidental bruchid parasite; and 5, 2 pteromalids including the cosmopolitan *Choetospilia elegans* Westwood.

To summarize, I have indicated various projects that deserve study; detailed rearings to assess infrapopulational variation as well as to precisely determine correct hosts—this is needed for all of the parasites, and studies of host induced variation should be easily accomplished because it should be possible to establish and maintain lab colonies of most of the parasite species; studies of particular host-parasite systems rich in parasites, notably the bruchids of the genus *Merobruchus* and host plants of the genera *Albizzia* and *Lysiloma*; and comparisons of host-parasite systems that probably involve sibling complexes, the most promising such system being that of *Amblycerus-Spilochalcis*.

Acknowledgments

G. Gordh, J. M. Kingsolver, P. M. Marsh, and A. S. Menke, Systematic Entomology Laboratory, ARS, USDA; T. L. Erwin, Smithsonian Institution; and D. H. Janzen, University of Michigan. I am deeply indebted to Gordh and Marsh for many hours spent on identifications of chalcidoids and braconids, respectively, and for constructive criticism; Marsh rewrote the braconid key to better distinguish genera. Menke identified the bethylid. Kingsolver and I share responsibility for bruchid determinations; Kingsolver and Erwin criticized the manuscript. Janzen supported the project by providing basic materials, commenting on the manuscript, and providing support from NSF grants GB 35032X and BMS 75-14268.

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THE AWARDS PROGRAM OF THE ACADEMY AND RECENT HONOREES

Three research scientists and two science teachers were recipients last Spring of the Academy's awards for outstanding scientific achievement. The presentations were made at the Annual Awards Dinner meeting of the Academy on March 20, 1975, at the Cosmos Club.

The following research investigators were honored: Dr. Floyd E. Bloom, Division of Special Mental Health Research, National Institute of Mental Health, in the Biological Sciences; Dr. John D. Anderson, Jr., Department of Aerospace Engineering, University of Maryland, in the Engineering Sciences; and Dr. David L. Griscom, Material Sciences Division, Naval Research Laboratory, in the Physical Sciences.

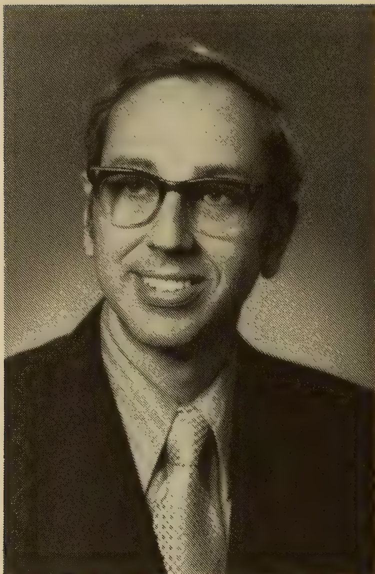
In the area of Teaching of Science, a joint award was presented to Dr. Carleton R. Treadwell, Professor & Chairman, Department of Biochemistry, The George Washington University School of Medicine, and to Dr. Donat G. Wentzel, Astronomy Program, University of Maryland.

Floyd E. Bloom

Biological Sciences

Floyd E. Bloom was cited for "the molecular mechanisms and the function of axodendritic noradrenergic synapses."

Dr. Bloom was born October 8, 1936 in Minneapolis, Minnesota. He received his A.B. degree in 1956, *cum laude*, from Southern Methodist University and his M.D. degree in 1960, *cum laude*, from Washington University School of Medicine. His internship and first year residency in medicine were spent at Barnes Hospital and Washington University School of Medicine, both in St. Louis, 1960–1962. His memberships in learned societies include the following: Phi Beta Kappa; Alpha Omega Alpha, Sigma Xi, and International Society for Sterology. Special awards received previously by Dr. Bloom are the A. E. Bennett Award (1971), A. Cressy Morrison Award (1971), Arthur S. Fleming Award (1972), and Mathilde Solowey Award (1973). Educational institutions with which he has held important teaching and research positions are St. Elizabeth Hospital; George Washington University School; Yale University School of Medicine; and Connecticut Mental Health Center. Since July 1973, Dr. Bloom has served as Acting Director, Division of Special Mental Health Research Programs, National Institute of Mental Health, St. Elizabeths Hospital, Washington, D. C.



John D. Anderson, Jr.

Engineering Sciences

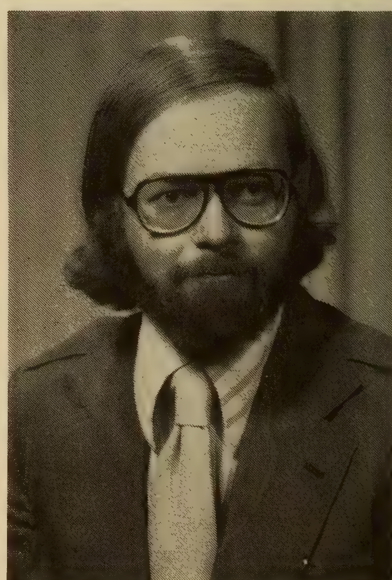
John D. Anderson, Jr., was cited for "major contribution to manned atmospheric entry and high energy lasers."

Dr. Anderson was born October 1, 1937 in Lancaster, Pennsylvania. He received the Bachelor of Aeronautical Engineering degree from the University of Florida with high honors in June 1959. His Ph.D. in Aeronautical and Astronautical Engineering was conferred by The Ohio State University in September 1966. His memberships in learned societies include the following: Tau Beta Pi, Sigma Xi, Sigma Tau, Phi Kappa Phi, and Phi Eta Sigma. Special academic honors received by Dr. Anderson are the J. Hillis Miller Memorial Scholarship (undergraduate); Institute of the Aeronautical Scholastic Branch Award (1959); Chicago Tribune Silver Award for Military Merit (1958); Nominee for Maryland's Outstanding Young Scientist of 1971 (Maryland Academy of Scientists); and Meritorious Civilian Service Award (December 15, 1972). Educational institutions with which he has held important positions are analytical engineer for Pratt and Whitney Aircraft in Hartford, Conn.; Chief, Hypersonics Group, Astrophysics Division, Naval Ordnance Laboratory; and part-time Lecturer, Mechanical Engineering, Catholic University of America. Since May 1973, Dr. Anderson has been Chairman and Professor, Department of Aerospace Engineering, University of Maryland, College Park, Maryland.

Physical Sciences

David L. Griscom was cited for "the imaginative use of microwave spectroscopy to characterize magnetic structures in amorphous solids."

Dr. Griscom was born November 1, 1938 in Pittsburgh, Pennsylvania. He received the B.S. degree from Carnegie Institute of Technology in 1960. His Ph.D. degree in Physics was conferred by Brown University in 1966. His memberships in learned societies include the following: American Physical Society, Sigma Xi, and American Ceramic Society. In 1971, he received an Outstanding Performance Rating at Naval Research Laboratory. The year following his completion of the doctorate in Physics at Brown University, he served as a Research Associate in Physics in



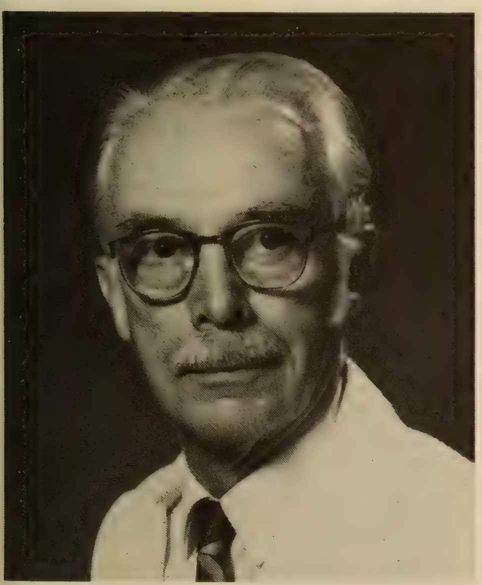
David L. Griscom

the same institution. Since July 1, 1971, Dr. Griscom has served as Head, Radiation Effects Section, Solid State Division, Naval Research Laboratory, Washington, D. C.

Teaching of Science
(Joint Award)

Medical School.—Carleton R. Treadwell was cited as a "teacher—dedicated, patient, concerned, always helpful—a man of integrity."

Dr. Treadwell was born December 28, 1911 in Calhoun County, Michigan. He received the A.B. degree from Battle Creek College in 1934. The M.S. and Ph.D. degrees were both conferred by the University of Michigan in 1935 and 1939, respectively. His professional activities include the following: Consultant in Medical Research, VA Center, Martinsburg, West Virginia, 1954 to date; Consultant in Biochemistry, Institute of Biochemistry, Walter Reed Army Medical Center, 1965 to date; Chairman, Gordon Research Conference on Lipids, 1959–60; and Editorial Board, JOURNAL OF NUTRITION, 1955–59. Dr. Treadwell has held the following positions: Teaching Fellow, University of Michigan, 1935–39; Baylor University School of Medicine (Instructor of Biochemistry, 1939–42; Assistant Professor, of Biochemistry, 1942–43); Associate Professor of Biochemistry, Southwestern Medical School, 1943–45; and George Washington University School of Medicine (Assistant Professor of Biochemistry, 1945–47; Associate Professor of Biochemistry, 1947–52; Professor of Biochemistry, 1952 to date). Since 1959, Dr. Treadwell has been Chairman, Department of Biochemistry, George Washington University School of Medicine, Washington, D. C.



Carleton R. Treadwell

Donat G. Wentzel

University.—Donat G. Wentzel was cited for "promoting improved Astronomy teaching on both college and secondary school levels."

Dr. Wentzel was born June 25, 1934 in Zurich, Switzerland. He received his B.A. degree, B.S. degree, M.S. degree and Ph.D. degree, all at the University of Chicago in 1954, 1955, 1956, and 1960, respectively. Educational institutions in which he has held positions are the following: University of Michigan (Assistant Professor, 1961–64 and Associate

Professor, 1964–66). Since 1974, he has been a Professor in the Astronomy Program at the University of Maryland, College Park, Maryland.

Some New Information About The Academy's Awards Program

The Board of Managers of the Academy has approved a sixth achievement award area, beginning with the 1975 Awards Program. It will be in the area of Behavioral Sciences. This new award will be restricted to the recognition of work involving objective, laboratory studies in the behavioral sciences.

A second new policy approved by the Board is that the Teaching of Science Award will be designated in the future as the Berenice G. Lamberton Award for the Teaching of Science. The Award will recognize teaching scholars at the college and high-school levels, respectively.—Kelso B. Morris, *General Chairman.*

BOARD OF MANAGERS MEETING NOTES

Feb. 11, 1975

The 628th meeting was called to order at 8:00 p.m. by President Stern in the Conference Room in the Lee Building at FASEB. The minutes of the previous meeting were corrected and approved.

Treasurer.—Dr. Rupp presented the annual report for 1974, and also presented a summary of the expenses of the past three years together with a balanced budget for the 1975 calendar year. His motion for acceptance of the annual report and the proposed budget was seconded by Dr. Sulzbacher and approved.

Tellers Committee.—Mr. Charles Rader announced the results of the election and recommended adoption of a plurality system for counting votes in the future. The following were elected for the 1975–76 session:

President-elect: Dr. Florence H. Forziati
Secretary: Dr. Alfred Weissler
Treasurer: Dr. Richard H. Foote
Managers-at-Large:
Dr. Howard Noyes
Dr. Leland Whitelock

Membership Committee.—Dr. Florence Forziati presented three nominees for Fellowship: Mr. Joseph F. Coates, Dr. Anne R. Headley, and Dr. Marion M. Schnepfe, and three new delegates: Dr. T. Cook, representing the American Society of Microbiology, Dr. Ralph Hudson, representing the Philosophical Society of Washington, and Mr. A. James Wagner, representing the American Meteorological Society. Her motion for acceptance of these six new Fellows was seconded by Dr. Rupp and approved.

Policy Planning/Ways and Means.—Dr. Alphonse Forziati presented the plans and the budget for the upcoming Symposium on "Energy Recovery From Solid Wastes," mentioning that all expenses would be covered.

Committee on Meetings.—Dr. Honig reported that the meeting at the Polish Embassy was a huge success. Two hundred people attended, and many more were disappointed because the Embassy wouldn't accommodate them. Dr. Stern mentioned that he had received a few letters from Polish Americans protesting the holding of a scientific meeting at the Embassy (not neutral territory).

Awards Committee.—Since Dr. Kelso Morris, Chairman of the Awards Committee, was absent, Dr. Stern presented his report. The following were nominated and approved by the Board:

Biological Sciences: Floyd E. Bloom, National Institute of Mental Health
Engineering Sciences: John D. Anderson, Jr., Univ. of Md.
Physical Sciences: David L. Griscom, Naval Res. Lab.
Mathematics: No Award given

Teaching of Science (joint award): Carleton R. Treadwell, The Geo. Washington Univ., and Donat G. Wentzel, University of Md.

A general discussion, regarding whether the establishment of separate awards for college and High School teaching should be considered, followed, with Dr. Robbins making a motion, seconded by Dr. Rupp, that separate awards be granted in the future, and that all nominees (losers) be invited to become Fellows of the Academy. This motion was approved by the Board.

Committee on Encouragement of Science Talent.—In Mrs. Shafrin's absence, Dr. Stern presented her report. Abstracts of most of the papers presented by the Junior Academy members at their annual Christmas convention will be published in the Spring issue of the Journal of the Academy.

Bicentennial.—Dr. Raymond Seeger's recommendation that the Academy concentrate on honoring the scientists in the Washington area who were important nationally in the development of science by setting up a committee of knowledgeable people who could speak about the accomplishments of a few particular scientists at each meeting, provoked a lengthy discussion. Other suggestions included concentrating on the next 100 years instead of the past, placing a column in the Journal describing how the affiliates were planning to celebrate the Bicentennial, inviting the affiliates to plan a joint celebration, having the theme for the 1976 symposium to be "The Bicentennial," drafting some historians of Science to help in the planning, describing how each President of the USA worked with Scientists, reviewing the religion of our forefathers, etc. Dr. Robbins suggested the theme "Past is Prologue," therein covering the historical perspective and getting the plans underway. Dr. Stern decided to let the Committee on Policy Planning/Ways and Means consider the matter.

Divisional Structure and Joint WAS-Affiliate Program: Public Understanding of Science.—Dr. Stern reported that only 14 responses had been received to date from the affiliate societies regarding the proposed divisional structure, but that all 14 were in agreement with the proposal—all affirmative. He mentioned that Dr. Henry Liers, NRL, had agreed to take the responsibility of both raising the money and formulating the programs for one year (50 programs) to be shown on TV. The national Bureau of Standards and the Walter Reed Army Institute of Research were both cited as good sources of films for the program. He stated that the divisional structure would certainly be an asset in operating this program effectively; contacting 40 different societies would be a great inconvenience.

Dr. Rupp moved that the Columbia Historical Society and the National Geographic Society both be incorporated under the Life Sciences. This motion was seconded by Dr. Robbins and approved.

Dr. Stern proposed that each division have an elected chairman to be a member of the executive committee as well as a member of the Board of Managers, the main purpose being to encourage communication between societies having mutual interests.

Dr. Honig suggested that each division formulate its objective and present it in writing to the Board. Dr. Recheigl suggested a period of experimentation before changing the structure of the Academy.

Dr. Robbins made a motion, seconded by Dr. Rupp, to adopt the divisional structure for a trial period of two years. Dr. Honig made a motion, seconded by Dr. Bickley, to table the motion.

Dr. Thomas Cook, representative from the American Society for Microbiology, stated that his society has approved of the Divisional Structure so the tally now stands at 15 for approval.—Mary H. Aldridge, *Secretary*.

Contributions in this section of your Journal are earnestly solicited. They should be typed double-spaced and sent to the Editor three months preceeding the issue for which they are intended.

AMERICAN UNIVERSITY

Horace Isbell has had a paper published in *Carbohydrate Research*, Vol. 39, C4-C7, (1975) entitled "Concurrent Oxidation and Reduction Reactions of Cyclohexanehexone, Rhodizonic Acid, and Tetrahydroxy-1,4-benzoquinone with Hydrogen Peroxide."

Leo Schubert attended a meeting at the Steering Committee of Project SEED of the American Chemical Society on 19 February 1975. The function of this committee is to set up programs for the "disadvantaged." Dr. Schubert chairs "Catalyst" of Project SEED. He has also received an appointment for 1975–77 on the Joint Board-Council Committee on Chemistry and Public Affairs of the American Chemical Society. Of the six appointments for this term, three are reappointments. The other two appointments are Drs. Glenn T. Seaborg and L. J. Tepley. Dr. Schubert has been reappointed to the Program Review Committee of the American Chemical Society for 1975.

DEPARTMENT OF AGRICULTURE

Karl H. Norris, Chief of the Instrumentation Research Laboratory, U. S. Department of Agriculture, Beltsville, Maryland, was chosen by the American Academy of Achievement as one of fifty giants of accomplishment from the nation's great fields of endeavor to receive the Golden Plate Award during the 14th annual Salute to Excellence weekend, June 26–28, at Evansville, Indiana.

A nationally recognized authority on instrumentation—especially well known for developing the light-transmittance technique which provides the basis for automatic egg grading equipment in use today—Mr. Norris is the recipient of the 1974 Cyrus Hall McCormick gold medal, the nation's top honor in agricultural science, presented by the American Society of Agricultural Engineers.

Dedicated to the inspiration of youth "to raise their sights high; to excel in their endeavors," the Academy annually honors "exemplars of excellence" in business, the sciences, the professions, entertainment, sports, journalism, the arts, and service to fellow men.

Over 150 national and state champion high school honor students joined the symposiums and other events during the weekend gathering. Dr. Michael DeBakey, Leon Jaworski, Lorne Greene, and Louis Nizer—as past Academy honorees—assisted in the presentation of awards at the Banquet of the Golden Plate.

DEPARTMENT OF INTERIOR

Honorary Membership was conferred on Captain **Clement Leinster Garner**, Captain, U. S. Coast & Geodetic Survey (Retired), by the American Congress on Surveying and Mapping for leadership and service in shaping the objectives of the ACSM Control Surveys Division and serving as its first elected Chairman. Presentation of the ACSM plaque was made on May 2, 1975, by ACSM officers at the home of Captain Garner.

Captain Garner was Chief of the Coast Survey's Division of Geodesy from 1937 till his retirement some years ago. His various assignments included hydrographic and topographic surveys, gravimetric determinations, first-order triangulation in various sections of the United States, and also the precise measurements of Pasadena Base Lines for use in determination of the velocity of light. He was commanding officer on Coast & Geodetic Survey vessels and engaged in military surveys during the first World War.

He is a charter member and life member of ACSM and holds bachelor of science (1907) and doctorate (1940) degrees in engineering from North Carolina State College.

Captain Garner is also a member of the American Society of Civil Engineers, Washington Academy of Sciences, Washington Society of Engineers, Philosophical Society of Washington, American Astronomical Society, American Geophysical Union, Society of American Military Engineers, American Association for Advancement of Science, and American Society of Photogrammetry.

NATIONAL INSTITUTES OF HEALTH

Robert L. Dedrick, chief of the Chemical Engineering Section, Biomedical Engineering and Instrumentation Branch, DRS, received the 1974 Food, Pharmaceutical, and Bioengineering Division Award of the American Institute of Chemical Engineers.

The award for "outstanding contributions . . . and professional leadership in biomedical engineering" included a plaque, a certificate, and a check for $1,000.

In his acceptance speech, Chemical Engineering and Cancer Research, Dr. Dedrick discussed the application of chemical engineering to the problem of extrapolating observations from one biological system to another with particular reference to environmental toxicology.

He has earned degrees from Yale, the University of Michigan, and the University of Maryland.

His publications include work in pharmacokinetics, adsorption kinetics, biomaterials, hemodialysis, and instrumentation.

Dr. Dedrick also holds a number of patents on devices and processes for dialysis and tissue culture.

Ronald B. Herberman has been appointed acting chief of the National Cancer Institute's newly established Laboratory of Immunodiagnosis in the Division of Cancer Biology and Diagnosis.

The new laboratory is primarily concerned with the characterization of antigens associated with tumor cells.

Dr. Herberman will direct research on immune responses to tumor-associated antigens in experimental animals and cancer patients.

He received a B.A. degree in 1960 from New York University and an M.D. degree in 1964 from the N.Y.U. School of Medicine.

In 1966 Dr. Herberman joined NCI as a clinical associate in the Immunology Branch, and from 1968 to 1971 he was a senior investigator in the branch.

In 1971 he was appointed head of the Cellular and Tumor Immunology Section, a position he held until his present appointment.

NEW FELLOWS

James F. Goff, Research Physicist and Branch Chief, Thermoelectric Properties Section, U.S. Naval Surface Weapons Ctr., Silver Spring, Md., in recognition of his contributions to and experiments in the field of the transport properties of semi-conductors and transition metals. *Sponsors:* Zaka I. Slawsky, George Abraham.

Raynor L. Duncombe, Director, Nautical Almanac Office, U. S. Naval Observatory, in recognition of his outstanding contributions to celestial mechanics and dynamical astronomy, his administrative skill and leadership as Director of the Nautical Almanac Office, U. S. Naval Observatory, and his participation in many national and international organizations in his fields of interest. *Sponsors:* Bancroft Sitterly, Charlotte M. Sitterly.

Nicolae Filipescu, Professor of Chemistry, The George Washington University, in recognition of his contribution to

photochemistry, in particular his research on energy transfer. *Sponsors:* C. R. Naeser, Theodore Perror, Robert C. Vincent.

Arthur S. Jensen, Senior Advisory Physicist, Westinghouse Systems Development Div., Baltimore, Md., in recognition of his contributions to basic electron physics and their applications to electron tubes and electron devices. *Sponsors:* Jenny E. Rosenthal, Richard Tousey.

Howard St. Claire Jones, Jr., Chief, Microwave Res. & Dev. Branch, Supervisory Physical Scientist, Harry Diamond Labs., for contributions to microwave component and antenna system design with particular emphasis on compact lightweight antenna arrays. *Sponsors*: Paul E. Landis, George Abraham.

Milton N. Kabler, Head, Insulator Physics Branch, Material Sciences Division, Naval Research Laboratory, in recognition of his contributions to solid state physics and in particular his researches in the area of optical properties and defects in insulating crystals. *Sponsors:* A. I. Schindler, L. Teitler, George Abraham.

William V. Loebenstein, research chemist, Dental & Medical Material Sciences, Polymers Div., NBS, in recognition of his significant contributions to the understanding of the surface chemistry of tooth structure and restorative materials. The results of his work provide some of the understanding necessary to solve problems related to tooth decay, strengthening tooth structure to resist decay, and the tooth-restoration interface to enhance the adhesion of restorative materials. *Sponsors:* Nelson W. Rupp, George C. Paffenbarger, George Dickson.

Joseph M. Marchello, Provost, Division of Mathematical & Physical Sciences & Engineering, Univ. of Maryland. *Sponsors:* George Abraham, Alphonse F. Forziati, Sidney Teitler.

OBITUARIES

Alden H. Emery

Alden H. Emery, chief administrator of the American Chemical Society from 1946 to 1966, died in Suburban Hospital after a long illness. He lived on Park Crest Drive in Silver Spring.

Emery, a member of the American Chemical Society since 1923, joined the organization's staff as assistant manager in 1936. He became assistant secretary in 1943, secretary in 1946 and executive secretary in 1947.

Because of Emery's leadership the ACS kept abreast of the "scientific information explosion" and became the largest chemical society, perhaps the most effective technical society in existence, said Glenn T. Seaborg, president-elect of the society.

In 1961 Emery was awarded the society's gold medal and was cited for administering his office with "exceptional intelligence, tact, vision and responsiveness to the desires of the members."

Emery was born in Lancaster, N. H. A graduate of Oberlin College, he received an M.A. degree from Ohio State University. In the early 1920s he worked for the U. S. Bureau of Mines, becoming assistant chief engineer of the experiment stations division here.

Emery also worked on a number of publications of the ACS, including "Chemical Abstracts" and "Metallurgical Abstracts."

He was a member of the American Association for the Advancement of Science, the Washington Academy of Sciences and the Cosmos, Torch and University Clubs here.

He leaves his wife, Dorothy R.; two sons, Alden H., Jr., of Lafayette, Ind., and Robert W., of Lancaster, Pa.; and five grandchildren.

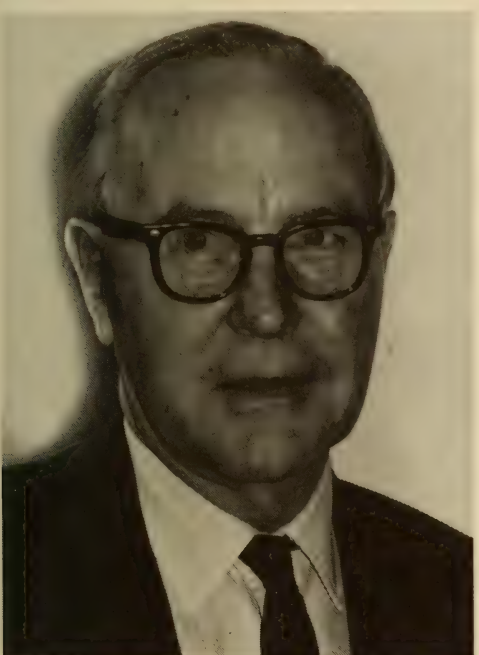
Malcom Colby Henderson

Malcom Colby Henderson, 71, a noted physicist, former Atomic Energy Commission official and Catholic University professor, died on July 18, 1975, in Berkeley, Calif., after a long illness. He moved to Berkeley in 1970 after retiring from Catholic University, where he had been a research physicist since 1954.

Known equally for his work in intelligence and for his accomplishments in the field of physics, Dr. Henderson was an outspoken critic of what he felt was unwarranted government secrecy in some areas.

He also carried the banner in other causes, such as the controversy at the Cosmos Club over the admittance of a Negro in 1962 and the 1967 massive rebellion at Catholic University over the firing of a teaching priest.

Dr. Henderson's work in the field of physics involved cyclotron design, artificial radioactivity, transmission and reception of underwater sound, atomic energy, ultrasonics and thermal relaxation in gases. Considered an authority



Malcolm C. Henderson

in these areas, he had served as a consultant to the National Science Foundation and did civilian work with the Office of Scientific Research and Development. He also entered into other fields.

Born in New Haven, Conn., he was a graduate of Phillips Academy at Andover and Yale University, where he was elected to Phi Beta Kappa.

Dr. Henderson then entered Cambridge University in England, where he studied under Lord Rutherford at the Cavendish Laboratory and received a doctorate in nuclear physics in 1928.

For the next four years, he was a Sterling Fellow and Honorary Research Fellow at Yale. From there he went to the University of California at Berkeley, where he assisted Ernest Lawrence in building the first cyclotron.

From 1935 to 1940, Dr. Henderson was an instructor in physics at Princeton University. He moved from there to Dartmouth College as a professor.

During World War II, he headed a group at the Navy Radio and Sound Laboratory in San Diego, which developed FM Sonar. This device was used by American submarines to detect enemy mines. It enabled the submarines to penetrate the Sea of Japan in 1945 through heavily mined straits.

After the war, Dr. Henderson served as a research analyst in the Intelligence Division of the Army for three years.

From 1949 to 1953, he was deputy director of the Office of Intelligence of the Atomic Energy Commission. This was followed by a year as director of atomic test operations for the Federal Civil Defense Administration before he joined the faculty of Catholic University.

It was after he had left the AEC and the Civil Defense Agency that Dr. Henderson spoke out strongly on secrecy in government. He voiced heavy opposition to restricting unclassified technical information.

"Suppressing information that is not classified will gain us nothing, jeopardize our precious liberties and impede technical progress," he declared. It was during a period of heavy debate on national

security and the right of the people to know about what its government was doing.

On another occasion he said that there was no question that present government security regulations were impeding exchange of scientific information and productive ideas between scientists.

"Let us have restrictions on classified information and let us put teeth in the law so we can prosecute and convict those who leak classified material, but let's put no faith in a general atmosphere of secrecy in a gray area," he told a meeting of the American Society of Newspaper Editors.

Dr. Henderson, a member of the prestigious Cosmos Club, entered the controversy in 1962 when the Club refused to admit Carl Rowan, a Negro and then Deputy Assistant Secretary of State for Public Affairs. While he was not among the members to resign, Dr. Henderson offered a resolution, adopted by the club, that would have banned exclusion of any person from membership on account of religion, color, race or national origin. Negroes are now admitted to the club.

The Catholic University rebellion was started by both cleric and lay students after the board of trustees decided not to renew the contract of the Rev. Charles E. Curran, an assistant professor of moral theology. He was known for his liberal views on such touchy matters as birth control and a new approach to morality.

Both the cleric and the lay faculty went out on strike. Dr. Henderson, as chairman of the Assembly of Ordinary Professors, led the lay faculty. The issue became an overall issue of teaching freedom.

The stakes were high, but the issue was finally resolved with concessions from the board.

Dr. Henderson was a former president of the Washington Philosophical Society and the Washington Academy of Science. He belonged to the Society of the Cincinnati.

He is survived by his wife, Katherine Linforth Henderson, of Berkeley; two sons, Ian Yandell Henderson, of Louisville, and Anthony Gordon Henderson, of New York City, and three grandchildren.

Hugh L. Logan

Hugh L. Logan, 74, retired physicist from the National Bureau of Standards and an internationally known authority on stress corrosion cracking, died June 23, 1975 after an illness in Arlington, Virginia. A native of Colorado, Mr. Logan joined the National Bureau of Standards in 1936 and remained there until his retirement in 1966. He obtained a BS in chemistry from Tarkio College, Tarkio, Missouri and an MS in physics at the University of Colorado. He had completed all requirements for the Ph.D. degree in physics at the University of Colorado with the exception of two courses when the opportunity to come to the Bureau arose in 1936. He retired in 1967.

His major work at the National Bureau of Standards was concerned with stress corrosion cracking. During his career, he became one of the outstanding workers in this important field. In 1952, he proposed the film rupture theory of stress corrosion, and over the years this theory has seen increasing acceptance. It is considered one of the major mechanisms for stress corrosion cracking. He is the author of the book "Stress Corrosion of Metals." This is the only book on the subject by a single author and is used extensively by corrosion engineers and metallurgists. He organized, along with Dr. E. H. Phelps of the United States Steel Corporation, a symposium on stress corrosion at the 2nd International Congress on Metallic Corrosion held in New York in 1963. He published over thirty papers and books.

He was a member of the National Association of Corrosion Engineers, Electrochemical Society, and American Society for Metals, and a fellow of the Washington Academy of Sciences. As recognition for his outstanding achievements, he received the Silver Medal of the Department of Commerce in 1960,

and he was the recipient of the Burgess Memorial Award of the Washington Chapter of the American Society of Metals in 1964.

He is survived by his wife, Ethel, a son, Hugh, Jr., and a grandson.

Howard S. Rappleye

Howard Snyder Rappleye, 83, for many years an authority on precise leveling for the U. S. Coast and Geodetic Survey, has died of cancer in Providence Hospital.

Rappleye was a treasurer of the Washington Academy of Sciences and was editor of the journal of the Congress of Surveying and Mapping—both for periods of 10 years.

His long association with the USCGS began after he left the Army as a captain at the end of World War I. About 1930 he was named chief of the section of precise leveling, a position he held until his retirement in 1954.

When the White House was renovated during the Truman administration, Rappleye was consulted to insure that the building remained balanced while the extensive construction program progressed.

During most of the last 30 years Rappleye devoted his summer vacations to teaching precise leveling at summer camps for surveying students from several northeastern universities.

Rappleye's numerous publications include two definitive government technical manuals.

A native of Ithaca, N. Y., Rappleye attended Cornell University there and also studied at New York and George Washington Universities.

He was a Mason and a Shriner and a member of Takoma Park Baptist Church, the Cosmos Club here and many professional societies.

His wife, the former Nettie Brewer, died two years ago. He leaves a son, Robert, of College Park, a botany professor at the University of Maryland, and two grandchildren.

Nathan Raymond Smith

On June 26, 1974, Nathan Raymond Smith died in Sarasota, Florida. Born in Whitehall, New York, on September 10, 1888, to Minnie F. and Frederick Smith, he became a Vermonter at the age of eight when his family moved to Benson, Vermont, and then, two years later, to a 500-acre hilltop farm near Ludlow in that state. In 1921 he married Katherine Reynolds of Texas. Both Dr. Smith and his wife (who died in 1959) were enthusiastic gardeners, and a beautiful garden was always part of their home.

Throughout his life, Dr. Smith retained a close association with his former classmates (Class of 1911) at the University of Vermont and with Benjamin Franklin Lutman, his bacteriology professor. For over 20 years Dr. Smith served as an officer of the Vermont Society in Washington, D. C. From 1911 to his retirement in 1951, with the exception of two years (1917 to 1919) of Army service at the Ford Laboratory in Detroit, Dr. Smith worked in Washington at the Bureau of Plant Industry, U. S. Department of Agriculture, on the microflora of the soil, life cycles of bacteria, and the decomposition of organic matter in the soil.

Nathan Raymond Smith

In the mid-thirties, encouraged by Dr. Charles Thom, his chief, he began a taxonomic project on the genus *Bacillus* that culminated in 1952 in the publication of *Agriculture Monograph No. 16,* which served as a basis of the section on the genus *Bacillus* in three editions of *Bergey's Manual of Determinative Bacteriology*. Of the 1134 strains covered, 1114 strains, which had upon receipt borne 158 different species names, were assigned to 19 species.

Coincident with his retirement from the USDA, Dr. Smith became a member of the Board of Trustees of Bergey's Manual and, with Drs. R. S. Breed and E. G. D. Murray, edited the 7th edition of the Manual. It was a busy ("every mail brought a letter, and sometimes two, from Dr. Breed"), enjoyable association. After Dr. Breed's death in 1956, Dr. Smith assumed the burden of correcting the proofs and examined every word under a reading glass. He was justly proud of the conclusion of Dr. L. W. Pari in his review of "Bergey's Seventh" (Science, 1958, *127*: 1403) that "The authors are to be congratulated on a superb task well done."

Dr. Smith was a member of the Society of American Bacteriologists (later the American Society for Microbiology), and was president of the Washington Branch in 1941–42. He was also a member of the American Association for the Advancement of Science, the Soil Science Society, the Society of Agronomists, and the Botanical Society of Washington. He served the Washington Academy of Sciences successively as vice president, corresponding secretary, archivist, and president. His presidency of the Academy in 1951 was a pleasant climax to his career in Washington.—Ruth E. Gordon

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CONTENTS

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FEATURE

The Operational Basis and Mathematical Derivation of the Gibbs Differential Equation[1]

George Tunell

Department of Geological Sciences, University of California, Santa Barbara 93106

ABSTRACT

Several authors have correctly indicated that the Gibbs differential equation (Gibbs's equation (12)) for an open system is a generalization of the Clausius differential equation for a closed system. The authors of all of the textbooks of thermodynamics with which I am acquainted that have discussed Gibbs's equation (12) have accepted it without attempting to supply an operational basis for it. However, Gibbs himself gave an excellent statement of the operational basis of his equation (12) on pages 140–141 of his memoir in Volume 3 of the Transactions of the Connecticut Academy of Arts and Sciences entitled "On the Equilibrium of Heterogeneous Substances," and from this statement his equation (12) is mathematically simply and easily derivable.

It is an honor and a privilege to be here and to present the second annual J. Willard Gibbs lecture. The purpose of this lecture is to show that the Gibbs differential equation, which is the basic equation of chemical thermodynamics, has a very simple operational basis and that from the experimentally determinable relations the Gibbs differential equation can be obtained by a simple mathematical transformation.

Several authors (Guggenheim, 1950, p. 17; Keenan, 1948, p. 449; Moelwyn-Hughes, 1957, p. 282–283) have correctly indicated that the Gibbs differential equation for an open system is a generalization of the Clausius differential equation for a closed system. Lynde Phelps Wheeler (1952, p. 70–71) in his excellent biography of Gibbs stated:

"This fundamental equation [i.e. the Clausius differential equation] formed the starting point for Gibbs' development and extension of thermodynamics. In a sense it may be said to embody the whole of his indebtedness to his predecessors. No one had in the slightest degree anticipated the line of his further development of the subject. Prior to him no one had realized that the equation could be generalized to include non-homogeneous bodies, or had seen that when so expanded it would hold the key to the great domain of chemical equilibrium. The story of how Gibbs was led step by step with inexorable logic to his great generalization and the completeness with which he explored its consequences form a narrative almost unique in the history of science. 'On the Equilibrium of Heterogeneous Substances' appeared upon the scientific horizon in the 1870's as unheralded as had Carnot's *Réflexions* in the 1820's; but whereas

[1] Second annual J. Willard Gibbs Lecture presented before the Washington Academy of Sciences on February 20, 1975.

Carnot's work required that of Kelvin and Clausius to bring it to fruition, Gibbs' work forms a completed whole in whose framework the developments of the succeeding three-quarters of a century in the fields it covers appear for the most part as necessary and inevitable consequences.''

If the amount and kind of matter in a homogeneous mass is considered to be fixed, its energy ϵ is a function of its entropy η and its volume v, and the differentials of these quantities are subject to the relation

$$d\epsilon = td\eta - pdv, \qquad (11)^2$$

where t denotes the absolute thermodynamic temperature and p denotes the pressure; this is the Clausius differential equation for a closed system in the notation of Gibbs.

The generalization of this equation that was required in the case of a homogeneous body of variable composition and variable mass was stated by Gibbs (1874–78, p. 116 or 1928, p. 63) in the following way: "But if we consider the matter in the mass as variable, and write $m_1, m_2, \ldots m_n$ for the quantities of the various substances $S_1, S_2, \ldots S_n$ of which the mass is composed, ϵ will evidently be a function of $\eta, v, m_1, m_2, \ldots m_n$ and we shall have for the complete value of the differential of ϵ

$$d\epsilon = td\eta - pdv + \mu_1 dm_1 + \mu_2 dm_2 \ldots + \mu_n dm_n, \qquad (12)$$

$\mu_1, \mu_2, \ldots \mu_n$ denoting the differential coefficients of ϵ taken with respect to $m_1, m_2, \ldots m_n$.'' This statement appears on page 116 of Gibbs's memoir entitled "On the Equilibrium of Heterogeneous Substances'' in Volume 3 of the Transactions of the Connecticut Academy of Arts and Sciences.

The authors of all of the textbooks of thermodynamics with which I am acquainted that have discussed Gibbs's equation (12) have accepted it without attempting to supply an operational basis for it (Finkelstein, 1969, p. 84; Fleury and Mathieu, 1954, p. 286; Guggenheim, 1950, p. 449; Kirkwood and Oppenheim, 1961, p. 52; Moelwyn-Hughes, 1957, p. 283; Partington, 1950, p. 106; Prigogine, Defay, and Everett, 1954, p. 67; Sommerfeld, Bopp, Meixner, and Kestin, 1956, p. 87; Wall, 1965, p. 189). Thus, for example, Prigogine, Defay, and Everett (1954, p. 66) state that: "For closed systems the first law of thermodynamics establishes the existence of the function of state U [this is the same as Gibbs's ϵ]. We now *presume* that this function must also exist when *the number of moles varies in an arbitrary manner* [italics by Prigogine, Defay, and Everett].'' However, in the abstract of his memoir "On the Equilibrium of Heterogeneous Substances'' that Gibbs prepared for the American Journal of Science he did not state that ϵ will evidently be a function of $\eta, v, m_1, m_2, \ldots m_n$; on the contrary he stated (Gibbs, 1878, p. 444, or Gibbs, 1928, p. 357) that in the case of a homogeneous body of variable composition and variable mass "It is easily shown that ϵ is a function of $\eta, v, m_1, m_2, \ldots m_n$, and that the complete value of $d\epsilon$ is given by the equation

$$d\epsilon = td\eta - pdv + \mu_1 dm_1 + \mu_2 dm_2 \ldots + \mu_n dm_n \quad .''$$

Furthermore on pages 140–141 of his memoir in the Transactions of the Connecticut Academy of Arts and Sciences Gibbs (1874–78, p. 140–141, or 1928, p. 85) gave an excellent statement of the operational basis of his equation (12) in the following words: "As, however, it is only *differences* of energy and of entropy that can be measured, or indeed that have a physical meaning, the values of these quantities are so far arbitrary, that we may choose independently for each simple substance the state in which its energy and its entropy are both zero. The values of the energy and the entropy of any compound body in any particular state will then be

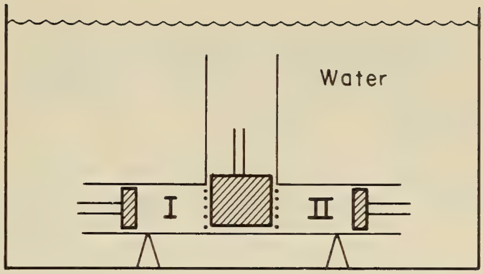
[2] Arabic numbers in parentheses are numbers of Gibbs's equations.

fixed. Its energy will be the sum of the work and heat expended in bringing its components from the states in which their energies and their entropies are zero into combination and to the state in question; and its entropy is the value of the integral $\int \frac{dQ}{t}$ for any *reversible* process by which that change is effected (dQ denoting an element of the heat communicated to the matter thus treated, and t the temperature of the matter receiving it).'' Thus he showed that the energy and the entropy of a compound body or solution can be obtained by measurements of the work done on a closed system and the heat received by the closed system provided that in the case of the determination of the value of the integral $\int \frac{dQ}{t}$ the reactions in the system take place in a reversible manner.

It remains to describe a concrete experimental method for carrying out the processes described by Gibbs, and to show that from the experimentally determinable functions, the Gibbs differential equation can be obtained by means of a mathematical transformation. Let us picture a constant temperature water bath. In this bath let us imagine a system of three chambers separated by semipermeable membranes as represented in Fig. 1.[3] The membrane separating chambers I and III is supposedly permeable only to component 1; similarly the membrane separating chambers II and III is supposedly permeable only to component 2. An arbitrary amount of component 1 and nothing else is placed in chamber I and an arbitrary amount of component 2 and nothing else is placed in chamber II. Chamber III is initially empty. All of the matter in the side chambers is then forced through the semipermeable membranes into chamber III by means of the pistons (Fig. 2). If the volume of chamber III has been calibrated in terms of the piston displacement, the volume of the solution v_{III} in chamber III is then determinable. By conducting a series of experiments we can obtain v_{III} as a function of the absolute temperature t, the pressure p, and the masses of the two components m_1 and m_2:

$$v_{\text{III}} = \Omega(t,p,m_1,m_2). \qquad (\text{I})^4$$

Also by determining the heat Q received from the water bath by the substances in the three chambers (a positive or negative quantity) and the total work W done by the three pistons on the substances in the three chambers (a positive or negative quantity), all of the mass of component 1 and all of the mass of component 2 being initially in the side chambers and finally in the central chamber, we can obtain the



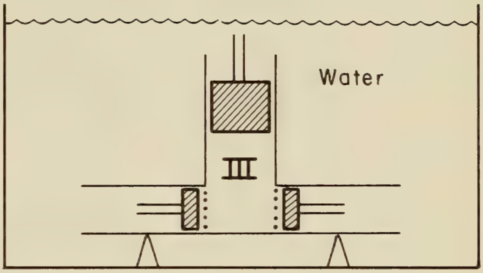


Fig. 1 (above). Thermostat containing a three-chamber system with semipermeable membranes and pistons in the initial position.

Fig. 2 (below). Thermostat containing a three-chamber system with semipermeable membranes and pistons in the final position.

[3] The use of semipermeable membranes was introduced into thermodynamics by Gibbs as J. R. Partington (1949, p. 163) has pointed out. Thus the use of a three-chamber system with semipermeable membranes to establish an operational basis for the Gibbs differential equation appears not to be inappropriate.

[4] Roman numbers in parentheses are numbers of equations in the derivations of the present author.

energy ϵ_{III} of the solution in chamber III in the final state as a function of the temperature, the pressure, and the masses of the two components by means of the relation

$$\epsilon_{\mathrm{III}}(t,p,m_1,m_2) - \epsilon_{\mathrm{I}}(t,p',m') - \epsilon_{\mathrm{II}}(t,p'',m'') = Q + W, \quad \text{(II)}$$

where p' and p'' denote the initial pressures in the side chambers I and II and p denotes the final pressure in the central chamber III, and ϵ_{I} and ϵ_{II} denote the energies of the masses of the pure components 1 and 2 in the side chambers I and II in the initial state. Finally, if the passage of the components through the semipermeable membranes is accomplished under equilibrium conditions by maintenance of the pistons at equilibrium osmotic pressures, in which case p' and p'' are functions of the temperature, the pressure, and the mass fraction of one component of the solution in chamber III, then the entropy η_{III} of the solution in chamber III in the final state can be obtained as a function of the temperature, the pressure, and the masses of the two components by means of the relation:

$$\eta_{\mathrm{III}}(t,p,m_1,m_2) - \eta_{\mathrm{I}}(t,p',m') - \eta_{\mathrm{II}}(t,p'',m'') = \int \frac{dQ}{t}, \quad \text{(III)}$$

where η_{I} and η_{II} denote the entropies of the masses of the pure components 1 and 2 in the side chambers in the initial state. Now if equations (I) and (III) can be solved for t and p as functions of η_{III}, v_{III}, m_1, m_2, we have

$$t = \Upsilon(\eta_{\mathrm{III}},v_{\mathrm{III}},m_1,m_2) \quad \text{(IV)}$$

and

$$p = \Phi(\eta_{\mathrm{III}},v_{\mathrm{III}},m_1,m_2). \quad \text{(V)}$$

Hence we have also

$$\epsilon_{\mathrm{III}} = \Theta(\eta_{\mathrm{III}},v_{\mathrm{III}},m_1,m_2). \quad \text{(VI)}$$

The total differential of ϵ_{III} is then given by the following equation

$$d\epsilon_{\mathrm{III}} = \left(\frac{\partial \epsilon_{\mathrm{III}}}{\partial \eta_{\mathrm{III}}}\right)_{v_{\mathrm{III}},m_1,m_2} d\eta_{\mathrm{III}} + \left(\frac{\partial \epsilon_{\mathrm{III}}}{\partial v_{\mathrm{III}}}\right)_{\eta_{\mathrm{III}},m_1,m_2} dv_{\mathrm{III}} + \left(\frac{\partial \epsilon_{\mathrm{III}}}{\partial m_1}\right)_{\eta_{\mathrm{III}},v_{\mathrm{III}},m_2} dm_1 + \left(\frac{\partial \epsilon_{\mathrm{III}}}{\partial m_2}\right)_{\eta_{\mathrm{III}},v_{\mathrm{III}},m_1} dm_2. \quad \text{(VII)}$$

From the Clausius differential equation for a system of constant mass and constant composition we know that

$$\left(\frac{\partial \epsilon_{\mathrm{III}}}{\partial \eta_{\mathrm{III}}}\right)_{v_{\mathrm{III}},m_1,m_2} = t \quad \text{(VIII)}$$

and

$$\left(\frac{\partial \epsilon_{\mathrm{III}}}{\partial v_{\mathrm{III}}}\right)_{\eta_{\mathrm{III}},m_1,m_2} = -p. \quad \text{(IX)}$$

By definition

$$\mu_1 \equiv \left(\frac{\partial \epsilon_{\mathrm{III}}}{\partial m_1}\right)_{\eta_{\mathrm{III}},v_{\mathrm{III}},m_2} \quad \text{(X)}$$

and

$$\mu_2 \equiv \left(\frac{\partial \epsilon_{\mathrm{III}}}{\partial m_2}\right)_{\eta_{\mathrm{III}},v_{\mathrm{III}},m_1} . \quad \text{(XI)}$$

Thus we obtain finally the result

$$d\epsilon_{\mathrm{III}} = t d\eta_{\mathrm{III}} - p dv_{\mathrm{III}} + \mu_1 dm_1 + \mu_2 dm_2 \quad \text{(XII)}$$

which is Gibbs's equation (12).

Now that the Gibbs differential equation has been derived, we turn to the question as to what measurements are necessary for the determination of complete thermodynamic information for a chemically variable system over a given range of temperature, pressure, and concentration.

Gibbs (1874–78, p. 143–144, or 1928, p. 88) stated that: "Any equation . . . between the quantities

$$\epsilon,\ \eta,\ v,\ m_1,\ m_2,\ \ldots\ m_n, \quad \text{(99)}$$

or $\psi, t, v, m_1, m_2, \ldots m_n,$ (100)

or $\chi, \eta, p, m_1, m_2, \ldots m_n,$ (101)

or $\zeta, t, p, m_1, m_2, \ldots m_n,$ (102)

or $t, p, \mu_1, \mu_2, \ldots \mu_n,$ (103)

is a fundamental equation, and any such is entirely equivalent to any other. For any homogeneous mass whatever, considered (in general) as variable in composition, in quantity, and in thermodynamic state, and having n independently variable components, to which the subscript numerals refer (but not excluding the case in which $n = 1$ and the composition of the body is invariable), there is a relation between the quantities enumerated in any one of the above sets, from which, if known, with the aid only of *general* principles and relations, we may deduce all the relations subsisting for such a mass between the quantities $\epsilon, \psi, \chi, \zeta, \eta, v, m_1, m_2, \ldots m_n, t, p, \mu_1, \mu_2, \ldots \mu_n$." Gibbs's functions ψ, χ, and ζ are defined by the following equations

$$\psi \equiv \epsilon - t\eta,$$

$$\chi \equiv \epsilon + pv,$$

$$\zeta \equiv \epsilon + pv - t\eta.$$

The question then arises: What measurements will suffice to permit the formulation of ϵ as a concrete function of $\eta, v, m_1, m_2, \ldots m_n$? We have already shown that if by means of measurements, v can be obtained as a concrete function of $t, p, m_1, m_2, \ldots m_n$, if ϵ can be obtained as a concrete function of $t, p, m_1, m_2, \ldots m_n$, if η can be obtained as a concrete function of $t, p, m_1, m_2, \ldots m_n$, and finally if the first and third of these relations can be solved for t and p as functions of $\eta, v, m_1, m_2, \ldots m_n$, then ϵ can be obtained as a concrete function of $\eta, v, m_1, m_2, \ldots m_n$.

The total volume v is equal to the specific volume $\breve{v}$ multiplied by the total mass

$$v = M\breve{v} \qquad \text{(XIII)}$$

where

$$M = m_1 + m_2 \ldots + m_n.$$

Likewise the total energy ϵ is equal to the specific energy $\breve{\epsilon}$ multiplied by the total mass

$$\epsilon = M\breve{\epsilon}, \qquad \text{(XIV)}$$

and the total entropy η is equal to the specific entropy $\breve{\eta}$ multiplied by the total mass

$$\eta = M\breve{\eta}. \qquad \text{(XV)}$$

The mass fractions $\breve{m}_1, \breve{m}_2, \ldots \breve{m}_{n-1}$ are defined by the equations

$$\breve{m}_1 \equiv \frac{m_1}{M}, \qquad \text{(XVI)}$$

$$\breve{m}_2 \equiv \frac{m_2}{M}, \qquad \text{(XVII)}$$

$$\vdots$$

$$\breve{m}_{n-1} \equiv \frac{m_{n-1}}{M}. \qquad \text{(XVIII)}$$

Consequently, if $\breve{v}$, $\breve{\epsilon}$, and $\breve{\eta}$ can be obtained as concrete functions of $t, p, \breve{m}_1, \breve{m}_2, \ldots \breve{m}_{n-1}$, then v, ϵ, and η can be calculated as functions of $t, p, m_1, m_2, \ldots m_n$.

In the case of a binary system of one phase it is known as a result of experiment that the pressure p, the specific volume $\breve{v}$, the absolute temperature t, and the mass fraction $\breve{m}_1$ of component 1 are connected by an equation of state

$$\Xi(p,\breve{v},t,\breve{m}_1) = 0 \qquad \text{(XIX)}$$

which can, in general, be solved for any one of these four quantities as a function of the other three.[5] From equation (II) it follows that the specific energy of a binary system of one phase is an experimentally determinable function of the absolute temperature t, the pressure p, and the mass fraction $\breve{m}_1$ of component 1. The partial derivatives of the specific energy with respect to temperature and pressure are known from the case of a system of constant composition and constant mass to be

[5] In certain cases multiple valued functions are encountered. For example, in the case of water, which has a minimum specific volume at 4°C, the temperature is expressed as a multiple valued function of the specific volume and pressure over certain ranges of specific volume and pressure.

$$\left(\frac{\partial \check{\epsilon}}{\partial t}\right)_{p,\check{m}_1} = \check{c}_p - p\left(\frac{\partial \check{v}}{\partial t}\right)_{p,\check{m}_1} \quad \text{(XX)}$$

and

$$\left(\frac{\partial \check{\epsilon}}{\partial p}\right)_{t,\check{m}_1} = \check{l}_p - p\left(\frac{\partial \check{v}}{\partial p}\right)_{t,\check{m}_1}, \quad \text{(XXI)}$$

where $\check{c}_p$ denotes the heat capacity at constant pressure per unit of mass and $\check{l}_p$ denotes the latent heat of change of pressure at constant temperature per unit of mass. Hence the relation of the specific energy of a binary system of one phase to the absolute temperature, the pressure, and the mass fraction of component 1 is expressed by the equation (Tunell, 1960, p. 8)

$$\check{\epsilon}(t,p,\check{m}_1) - \check{\epsilon}_0(t_0,p_0,\check{m}_{1_0}) = \int_{t_0,p_0,\check{m}_{1_0}}^{t,p,\check{m}_1} \left[\left\{\check{c}_p - p\frac{\partial \check{v}}{\partial t}\right\} dt + \left\{\check{l}_p - p\frac{\partial \check{v}}{\partial p}\right\} dp + \frac{\partial \check{\epsilon}}{\partial \check{m}_1} d\check{m}_1\right]. \quad \text{(XXII)}$$

Similarly it follows from equation (III) that the specific entropy of a binary system of one phase is an experimentally determinable function of the absolute temperature t, the pressure p, and the mass fraction $\check{m}_1$ of component 1. The partial derivatives of the specific entropy with respect to temperature and pressure are known from the case of a system of constant composition and constant mass to be

$$\left(\frac{\partial \check{\eta}}{\partial t}\right)_{p,\check{m}_1} = \frac{\check{c}_p}{t} \quad \text{(XXIII)}$$

and

$$\left(\frac{\partial \check{\eta}}{\partial p}\right)_{t,\check{m}_1} = \frac{\check{l}_p}{t}. \quad \text{(XXIV)}$$

Hence the relation of the specific entropy of a binary system of one phase to the absolute temperature, the pressure, and the mass fraction of component 1 is expressed by the equation (Tunell, 1960, p. 8)

$$\check{\eta}(t,p,\check{m}_1) - \check{\eta}(t_0,p_0,\check{m}_{1_0}) = \int_{t_0,p_0,\check{m}_{1_0}}^{t,p,\check{m}_1} \left[\frac{\check{c}_p}{t} dt + \frac{\check{l}_p}{t} dp + \frac{\partial \check{\eta}}{\partial \check{m}_1} d\check{m}_1\right]. \quad \text{(XXV)}$$

Necessary and sufficient conditions for equation (XXII) to be true are the following equations

$$\frac{\partial\left(\check{c}_p - p\frac{\partial \check{v}}{\partial t}\right)}{\partial p} = \frac{\partial\left(\check{l}_p - p\frac{\partial \check{v}}{\partial p}\right)}{\partial t}, \quad \text{(XXVI)}$$

$$\frac{\partial\left(\check{c}_p - p\frac{\partial \check{v}}{\partial t}\right)}{\partial \check{m}_1} = \frac{\partial \frac{\partial \check{\epsilon}}{\partial \check{m}_1}}{\partial t}, \quad \text{(XXVII)}$$

and

$$\frac{\partial\left(\check{l}_p - p\frac{\partial \check{v}}{\partial p}\right)}{\partial \check{m}_1} = \frac{\partial \frac{\partial \check{\epsilon}}{\partial \check{m}_1}}{\partial p}. \quad \text{(XXVIII)}$$

Likewise necessary and sufficient conditions for equation (XXV) to be true are the following equations

$$\frac{\partial \frac{\check{c}_p}{t}}{\partial p} = \frac{\partial \frac{\check{l}_p}{t}}{\partial t}, \quad \text{(XXIX)}$$

$$\frac{\partial \frac{\check{c}_p}{t}}{\partial \check{m}_1} = \frac{\partial \frac{\partial \check{\eta}}{\partial \check{m}_1}}{\partial t}, \quad \text{(XXX)}$$

and

$$\frac{\partial \frac{\check{l}_p}{t}}{\partial \check{m}_1} = \frac{\partial \frac{\partial \check{\eta}}{\partial \check{m}_1}}{\partial p}. \quad \text{(XXXI)}$$

From equations (XXVI), (XXVII), (XXVIII), (XXIX), (XXX) and (XXXI) it follows that

$$\check{l}_p = -t \frac{\partial \check{v}}{\partial t}, \qquad \text{(XXXII)}$$

$$\frac{\partial \check{c}_p}{\partial p} = -t \frac{\partial^2 \check{v}}{\partial t^2}, \qquad \text{(XXXIII)}$$

$$\frac{\partial^2 \check{\epsilon}}{\partial t \partial \check{m}_1} = \frac{\partial \check{c}_p}{\partial \check{m}_1} - p \frac{\partial^2 \check{v}}{\partial \check{m}_1 \partial t}, \qquad \text{(XXXIV)}$$

$$\frac{\partial^2 \check{\epsilon}}{\partial p \partial \check{m}_1} = -t \frac{\partial^2 \check{v}}{\partial \check{m}_1 \partial t} - p \frac{\partial^2 \check{v}}{\partial \check{m}_1 \partial p}, \qquad \text{(XXXV)}$$

$$\frac{\partial^2 \check{\eta}}{\partial t \partial \check{m}_1} = \frac{1}{t} \frac{\partial \check{c}_p}{\partial \check{m}_1}, \qquad \text{(XXXVI)}$$

and

$$\frac{\partial^2 \check{\eta}}{\partial p \partial \check{m}_1} = - \frac{\partial^2 \check{v}}{\partial \check{m}_1 \partial t}. \qquad \text{(XXXVII)[6]}$$

Therefore in order to have complete thermodynamic information for a binary system of one phase over a given range of temperature, pressure, and concentration, it would suffice to determine $\check{v}$ experimentally as a function of t, p, and $\check{m}_1$, then to determine $\check{c}_p$ experimentally at all points in a plane at constant pressure p', and to determine $\frac{\partial \check{\epsilon}}{\partial \check{m}_1}$ experimentally along a line at constant temperature, t', and constant pressure, p', and finally to determine $\frac{\partial \check{\eta}}{\partial \check{m}_1}$ experimentally along a line at constant temperature, t', and constant pressure, p' (Tunell, 1960, p. 12). By means of calorimetric measurements the necessary values of $\check{c}_p$ could be obtained. The values of $\frac{\partial \check{\epsilon}}{\partial \check{m}_1}$ could be determined over the range of concentration of interest at one temperature and one pressure by measurements of the heat transferred from the water bath to the substances in the three chambers (a positive or negative quantity) and the work done by the three pistons on the substances in the three chambers (a positive or negative quantity) in a series of experiments in which different proportions of the substances 1 and 2 are combined to form a single phase in the central chamber. The determination of $\frac{\partial \check{\epsilon}}{\partial \check{m}_1}$ at constant temperature and constant pressure over the range of concentration of interest could also be accomplished in many cases by means of a constant volume calorimeter, since the determination of the energy does not require that the components be combined in an equilibrium manner. The values of $\frac{\partial \check{\eta}}{\partial \check{m}_1}$ could be determined over the range of concentration of interest at one temperature and one pressure by measurements of the heat transferred from the water bath to the substances in the three chambers (a positive or negative quantity) in a series of experiments in which different proportions of the substances 1 and 2 are combined to form a single phase in the central chamber with maintenance of the pistons at equilibrium osmotic pressures. However, since Gibbs (1874–78, p. 138, or 1928, p. 83) proved that the thermodynamic condition of osmotic equilibrium in the case of a component present on both sides of a semipermeable membrane, and which can pass through the membrane, is the equality of its chemical potentials on both sides of the membrane, it would be possible in the following way to determine the values of $\frac{\partial \check{\eta}}{\partial \check{m}_1}$ without measurements of the heat transferred to the substances in the three chambers under osmotic equilibrium conditions. It is assumed that the thermodynamic properties of the pure substances in the side chambers have been determined. Measurement of the osmotic pressures across the two membranes would then give the values of μ_1 and μ_2

$$\mu_1(t,p,\check{m}_1) = \check{\zeta}_1(t,p') \qquad \text{(XXXVIII)}$$

[6] Tunell, 1960, p. 10–11.

and

$$\mu_2(t,p,\check{m}_1) = \check{\zeta}_{\mathrm{II}}(t,p''), \quad \text{(XXXIX)}$$

where $\check{\zeta}_{\mathrm{I}}$ and $\check{\zeta}_{\mathrm{II}}$ denote the specific zeta functions of substances 1 and 2. Gibbs (1874–78, p. 143 or 1928, p. 87) proved that

$$\zeta = \mu_1 m_1 + \mu_2 m_2. \qquad \text{(XL)}$$

From this it follows that

$$\check{\zeta} = \mu_1 \check{m}_1 + \mu_2 \check{m}_2. \qquad \text{(XLI)}$$

Thus if $\check{\epsilon}$ and $\check{v}$ had been determined as functions of t, p, and $\check{m}_1$, the value of $\check{\eta}$ could be calculated from the relation

$$\check{\eta} = \frac{\check{\epsilon} + p\check{v} - \check{\zeta}}{t}. \qquad \text{(XLII)}$$

When $\check{v}$, $\check{\epsilon}$, and $\check{\eta}$ have been determined as concrete functions of t, p, $\check{m}_1$, then by mathematical transformations ϵ can be obtained as a concrete function of η, v, m_1, m_2, likewise ψ can be obtained as a concrete function of t, v, m_1, m_2, also χ can be obtained as a concrete function of η, p, m_1, m_2, and ζ can be obtained as a concrete function of t, p, m_1, m_2, and finally p can be obtained as a concrete function of t, μ_1, μ_2.

In practice measurements of osmotic pressure have not been found as useful in general in the determination of the values of the chemical potentials as measurements of the electromotive force in suitable galvanic cells (Lewis and Randall, 1923, p. 263–273, and Tunell, 1960, p.12–15). However, the establishment of the operational basis of Gibbs's equation (12) by means of the system of three chambers that I have described appears to be in accord with Gibbs's statement of the general method for obtaining the energy and entropy of a compound body on pages 140–141 of volume 3 of the Transactions of the Connecticut Academy of Arts and Sciences, whereas the determination of the chemical potentials by means of galvanic cells does not directly correspond to this statement of Gibbs.

In conclusion I would say that since in his own abstract of "On the Equilibrium of Heterogeneous Substances" Gibbs wrote "It is easily shown that ϵ is a function of η, v, m_1, m_2, ... m_n, and that the complete value of $d\epsilon$ is given by the equation

$$d\epsilon = t d\eta - p dv + \mu_1 dm_1 + \mu_2 dm_2 \ldots + \mu_n dm_n,"$$

it seemed to me that it should be possible to derive this equation from the first and second laws applied to a concrete system. It is my hope that the explanation of the operational basis and mathematical derivation of the Gibbs differential equation which I have presented may be helpful in the future to some students of physical chemistry and geochemistry.

Acknowledgment

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February 19......	**May Inscoe,** USDA, Agricultural Research Center "*Chemical Communication in Insects*"
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April 15	**Cyril Ponnamperuma,** University of Maryland "*The Origin of Life*"
May 20	**George Abraham,** President, Washington Academy of Sciences Annual Dinner Meeting

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PROFILE

Raleigh Gilchrist (1893–1966)—American Pioneer in Platinum Metal Research

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ABSTRACT

An account of Raleigh Gilchrist's professional career and accomplishments and their significance is given. An extensive list of his publications is also presented.

In my biographical studies I have always been fascinated by the circumstances surrounding a given scientist's embarking on the field of research with which his name is usually associated (1). Often the choice seems purely fortuitous as was the case with Raleigh Gilchrist, one of America's most prominent internationally known authorities on the analytical chemistry of the platinum group metals and of gold. During the bitter cold winter of 1917–1918 Gilchrist, then a private in the U.S. Army Infantry, was undergoing training at Charlotte, North Carolina in a military police unit to be sent to France. Fortunately, by that time the Army was beginning to utilize the technical training of its recruits in its choice of assignments, and Gilchrist, whose graduate studies in chemistry at Cornell University had been interrupted by the military draft, suddenly received orders to report for duty on January 28, 1918 to the Nitrate Division of the Ordnance Corps of the U.S. Army in Washington, D.C. Here he was dispatched to work under Dr. William Francis Hillebrand (1853–1925), former chemist for the U.S. Geological Survey and at that time Chief of the Chemistry Division of the U.S. National Bureau of Standards (1908–1925).

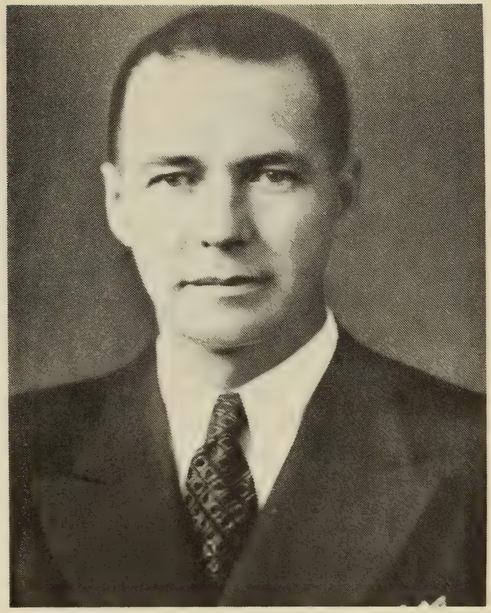
At NBS Gilchrist was assigned the task of determining the effect of differences in composition of platinum catalysts on the efficiency of the Ostwald process for oxidizing ammonia to nitric acid. The analysis of platinum metal alloys was then a problem of exceptional difficulty, and Gilchrist devoted his entire career—45 years at NBS—to this challenging work. It was fitting that in 1938, twenty years after his advent to the Bureau, he was awarded the Chemical Society of Washington's Hillebrand Prize for his work in this area, for Dr. Hillebrand had initiated this research at NBS.

Raleigh Gilchrist was born in the small town of Windsor, Vermont on January 8, 1893, the youngest of three sons of Hugh and Ella Gilchrist (née Renfrew). The

Gilchrists belonged to the MacLachlan and Olgilvie clans, while the Renfrews came from Renfrewshire near Glasgow. Gilchrist, or ''Gil'' as he was known to his colleagues, was always proud of his Scottish heritage and in later years was active in preserving Scottish culture through his association with the St. Andrew's Society in Washington (Secretary, Second and First Vice-President). When he and his wife visited Scotland in 1952, he purchased a complete formal Highland outfit as well as the daytime dress (2). His forbears, both paternal and maternal, had emigrated to the United States in the seventeenth century, and some of them served in the American Revolution.

Gilchrist's father, who lived to the age of ninety, and his mother were both newspaper people, who met while working for the Vermont Journal in Windsor. In 1896 the Gilchrists moved to Great Falls, Montana, where the father worked as a reporter for the local newspaper. While attending high school, from which he graduated in 1910, young Gilchrist delivered the morning newspaper. In order to earn money to attend college, he began work in June, 1910 at the Boston and Montana Reduction Works of the Anaconda Copper Mining Company in Great Falls, where, as an assistant handling gold and silver slimes recovered from the electrolytic purification of copper, he earned $1.75 for an eight-hour day. In January, 1911 he became record and time keeper and assistant to the chief in charge of repairing the calcining, blast, converter, and reverberatory furnaces at $2.00 per day. Here he learned the art of fire assaying. In September, 1911 he entered the University of Montana at Missoula, from which he received his B.A. degree with a major in chemistry in 1915. He earned his college expenses by waiting on tables in the girls' dormitory during his first three years and by serving as a storeroom assistant in chemistry during his senior year.

In September, 1915 Gilchrist began graduate study at Cornell University with



Raleigh Gilchrist

an assistantship in qualitative analysis. One of his students, a freshman named Elizabeth Hodgson Reigart, was destined to become his wife ten years later, on January 4, 1925, in Ithaca. He majored in inorganic chemistry under the head of the department, Prof. Louis Munroe Dennis, nicknamed ''The King,'' and minored in physical chemistry under Prof. Wilder Dwight Bancroft (founder and first editor of the *Journal of Physical Chemistry*) and analytical chemistry under Dr. Gustav Ernst Fredrick Lundell. At Ithaca he had completed the requirements for his minors, passed his examinations in French and German, and begun his doctoral research on germanium compounds where he was inducted into the U.S. Army on November 12, 1917 and sent to Camp Dix at Wrightstown, New Jersey. He was soon transferred to Charlotte, North Carolina and thence on January 28, 1918 to the Inorganic Chemistry Section of the National Bureau of Standards in Washington, as related above. At the Bureau, Dr. Lundell, who had been one of his professors at Cornell, was then Head of the Section on Standard Samples and later became Chief Chemist.

In the Fall of 1918, the epidemic of so-called Spanish influenza hit the United States, resulting in about 500,000 deaths. Gilchrist contracted the disease, which was complicated by double pneumonia, requiring about a year's recuperation. As a soldier, he received excellent treatment at the Walter Reed Hospital. Had he been a civilian he probably would not have survived. On January 15, 1919 he was discharged as a sergeant from the Army and on the same day joined the National Bureau of Standards as a civilian with the rank of Assistant Chemist "to engage upon a program of investigation of the refining and analytical chemistry of the platinum group metals." Wishing to resume his graduate work and still remain at the Bureau, Gilchrist enrolled at the Johns Hopkins University in Baltimore, where he began a new doctoral research problem under Dr. Joseph Christie Whitney Frazer with minors in physical chemistry and physics. He was not required to take any class work, but he audited a course in colloid chemistry from Dr. Walter A. Patrick, who played an important role in the commercial production of silica gel. Gilchrist received his doctorate on June 13, 1922 with a dissertation "The Preparation of Pure Osmium and the Atomic Weight of Osmium," which was published in 1932 under the title "A New Determination of the Atomic Weight of Osmium" (6).

At NBS Dr. Gilchrist rose through the ranks to become Chemist in 1936. He became Chief of the Platinum Metals and Pure Substances Section of the Bureau's Division of Chemistry. From 1948 to 1961 he was Chief of the Inorganic Chemistry Section. He retired on November 30, 1962, after which he served as consultant to the Chief of the Chemistry Division. His duties initially consisted of planning and carrying out experimental chemical research on methods for preparing each of the six platinum metals in pure form and of developing a knowledge of their chemistry. He also tested for chemical composition a variety of precious metal alloys, materials, and articles from other governmental agencies, and he analyzed materials where disputes had arisen between commercial chemists. He developed methods for analyzing materials containing the platinum metals, gold, silver, and the base metals usually associated with them. He also worked on the purification of sulfur, nickel, zirconium, barium, strontium, germanium, and the rare earths as well as on the preparation of titanium halides and the analysis of ceramic dielectrics. The results of his systematic studies were of great practical value and have been utilized by the precious metals industry, displacing older, inadequate procedures, and by scientists in the aeronautical, dental, and industrial fields. He also supervised the preparation of pure substances and the testing of reagent chemicals.

In 1936, 1938, and 1948 Gilchrist was a member of the United States Assay Commission, which meets yearly at the U.S. Mint in Philadelphia to check coinage. He was an official United States delegate to a number of international conferences, including in 1934 the Third International Technical and Chemical Congress in the Agricultural Industries (Paris), the Eleventh Conference of the International Union of Chemistry (Madrid), and the Ninth International Congress of Pure and Applied Chemistry (Madrid). At the last-mentioned meeting, he presented the paper for which he was awarded the Hillebrand Prize four years later (7). In 1927–1928 and 1929–1934 he was a lecturer in chemistry (thermodynamics and advanced inorganic) at George Washington University and in 1928–1929, 1931–1932, and later years a lecturer in chemistry at the NBS Educational Schools. A prolific but meticulous writer, at NBS he taught in the 1950s a course in technical writing and served for more than thirty years as a member (later Chairman) of the Chemistry Division Editorial Committee. In 1950 he received the Meritorious Service Medal Award from the Department of Commerce and in 1966 the Alumni Achievement Award of the University of Montana.

A longtime member (almost half a century) of the American Chemical Society, Dr. Gilchrist held continuous office in the Chemical Society of Washington for 32 years (Secretary, 1925–1928; President, 1929). For his development of methods for analyzing gold dental alloys, he was elected a member of the International Association for Dental Research. He was also a member of numerous honorary, scientific, and fraternal organizations, including Phi Beta Kappa. He was interested in wild bird life and astronomy, and his hobbies included gardening, photography, bowling, ballroom dancing, and crossword puzzles. For many years, both he and his wife were Braille transcribers for the American Red Cross.

The Gilchrists were fond of travel and visited Europe five times (1926, 1934, 1952, 1954, and 1965) and South America once (1957). Three of the European trips were on official business. For ten weeks (September–November 1926), Dr. Gilchrist visited England, Belgium, Holland, Germany, Italy, Switzerland, and France "to establish friendly relations with foreign laboratories in which research on the platinum metals was being done, to become acquainted with professors working on these metals and to see as much of the great platinum works as possible and to discuss problems of mutual interest" (8). His two-month trip (March–May, 1934) has already been mentioned in connection with his serving as one of ten U.S. delegates to three international meetings. In 1954 he presented an invited paper, "L'Analyse chimique des métaux du groupe du platine" (9) before the Société Française de Metallurgie at Paris and also visited Switzerland, Spain, Mallorca, Portugal, and England in addition to France.

Although described by a colleague as "a dour Scotsman with a barrel chest and legs," Gilchrist had "a remarkable sense of humor and a huge collection of stories, which he was very gifted at telling," largely because of "his ability to mimic any accent" (2). He was said to be "an ageless soul who defies all pretense at convention, and appears at work in shorts if he chooses; and who is considered by his wife to be the most stubborn man in the world" (2). He was a perfectionist both in his work and in his avocations. After a short illness, he died in Washington on October 25, 1966 at the age of seventy-three.

Platinum Metals. During World War I the importation of Chilean saltpeter from the Atacama Desert was threatened by German submarines, and it became necessary to produce synthetically the nitric acid and nitrates needed for explosives. Because of the resulting intense interest in nitrogen fixation, Dr. William F. Hillebrand's dream of creating a laboratory to refine the platinum metals and to develop methods for determining the individual metals became a reality. Young Gilchrist was assigned to work on the analysis of platinum-iridium gauzes used to convert ammonia to oxides of nitrogen and eventually to nitric acid by the process invented by Ostwald in 1901. The assignment was soon widened in scope, and in the course of years, analyses at NBS under Gilchrist's supervision were made on "a wide variety of materials, ranging from the determination of the quantity and thickness of the gold wash on the inside of a cocktail goblet to that of the composition of the highly complex native grain platinum" (5). The preparation of the platinum metals in highly pure form for the determination of various physical properties such as atomic weight required additional treatment of the commercial metals, and this task was also within Gilchrist's purview.

Gilchrist found that most of the analytical methods in the literature for the platinum metals did not produce clean-cut separations, particularly the venerable precipitation of platinum with ammonium chloride and the extraction of metallic mixtures with acids or with molten pyrosulfate. One remarkable exception was the insolubility of iridium in molten lead, employed by Deville and Stas in analyzing the platinum-

iridium alloy used in fabricating the international prototype meter and kilogram (10). In this method the alloy is melted with ten times its weight of lead, producing alloys of lead and platinum, which are soluble in acids, and crystalline iridium, which is virtually insoluble in *aqua regia*. In his first publications (11, 12) Gilchrist confirmed the accuracy of the method and modified it to increase its ease and speed of operation. The method is an excellent one for routinely analyzing platinum alloys containing no Fe, Ru, or Os. Beamish (ref. 13, p. 127) recommends it for determining iridium in platinum or palladium alloys, in which the latter two noble metals are not to be determined. The method is readily adaptable to massive forms of alloys such as sheet or wire.

Weeks (14) has called ruthenium "the little Benjamin of the platinum family" because it saw the light of day so much later than its older brothers. Gilchrist devised a gravimetric method for this least-known and last-discovered member of the platinum metals group that involved the principle of controlled hydrolytic precipitation (15), a principle employed in crude form as early as 1835 by Döbereiner, who used lime water to isolate Pt from Os, Rh, Ir, Pd, and Cu (16). Gilchrist added enough $NaHCO_3$ to the solution to turn bromcresol indicator faintly purple, and he then boiled and filtered the solution. The precipitate was ignited in a H_2 atmosphere and weighed as metallic ruthenium. Gilchrist also found a delicate test for ruthenium—the deep red or rose color developed on heating when thiourea and a few drops of HCl are added to a neutral solution.

Although distillation of OsO_4, usually from a solution acidified with HNO_3, has been generally used to separate osmium from the other platinum metals, no study of the optimum conditions or completeness of separation was made until Gilchrist's study of 1931 (17). He found that the form in which the osmium exists in solution has a marked effect on the rate at which it is volatilized. He used a 6 *N* HCl solution saturated with SO_2 for absorbing the distilled OsO_4, and the osmium was recovered from this by hydrolytic precipitation with $NaHCO_3$. The hydrated OsO_2 was ignited in H_2 and CO_2 and weighed as metallic osmium. Gilchrist concluded that a complete separation of osmium from the other platinum metals is possible by the traditional method if proper precautions are observed. More recent investigations (18, 19) resulting in low values by use of Gilchrist's SO_2–HCl receiving solution have been shown to be due to aging of the solution rather than to actual loss of osmium (20).

During the period 1915–1943 only two chemical determinations of atomic weights of the platinum metals were made—osmium by Gilchrist (6) and ruthenium by Gleu and Rehm (21). From the osmium content of $(NH_4)_2[OsCl_6]$ and $(NH_4)_2[OsBr_6]$, Gilchrist, returning to the topic of his doctoral dissertation, proposed the value 191.5, one considerably below today's accepted value of 192.2, obtained from isotopic abundance ratios (22).

Gilchrist's work, however, was not limited to research on the individual platinum metals but encompassed the separation and determination of each of the metals in the presence of the others. An important step in this direction was his paper on "Purification of the Six Platinum Metals," published in 1928 (23). In 1923 Wada and Nakazono noted that rhodium is precipitated by the reducing action of $Ti_2(SO_4)_3$, while iridium is not, if the chloride solution of each metal is treated separately (24). Gilchrist developed this qualitative observation of selective reduction into a quantitative separation of the two metals and their gravimetric determination (25). The rhodium is redissolved in boiling H_2SO_4, reprecipitated by H_2S, ignited in H_2, and weighed as metal. The titanium, now in the tetravalent state, is precipitated from the filtrate with cupferron ($C_6H_5N(NO)ONH_4$). The iridium is precipitated from the filtrate hydrolytically with $NaHCO_3$, and the $IrO_2 \cdot nH_2O$ is ignited in H_2 and weighed as metal. The

recent chromatographic method of Rees-Evans *et al.* (26), modified by Payne (27), has been reported to be simpler than Gilchrist's titanium method.

In 1934 Gilchrist devised a procedure for the separation of ruthenium from platinum, palladium, rhodium, and iridium based on hydrolytic precipitation (28). In the same year he employed hydrolytic precipitation by $NaHCO_3$ (pH 6) in the presence of $NaBrO_3$ (which prevents hydrolysis of $PtCl_6^{2-}$) to remove Pd, Rh, and Ir from a solution containing Pt (29). The Pt remaining in solution is precipitated by H_2S, dissolved in aqua regia, precipitated with sodium formate, ignited, and weighed. Pd is precipitated with dimethylglyoxime (30), ignited, and weighed as metal, while Rh and Ir are determined gravimetrically by Gilchrist's earlier procedure (25).

By 1934 Gilchrist and his colleague Edward Wichers had developed a new and reliable procedure for the separation and gravimetric determination of all six platinum metals based largely on Gilchrist's work of the previous decade cited above. The method, which was simpler than the methods previously used, was discussed in the paper "A New System of Analytical Chemistry for the Platinum Metals" (7), presented by Gilchrist at the Ninth International Congress of Pure and Applied Chemistry, for which he and Wichers received the Hillebrand Prize (31). Os and Ru are first removed by distillation, and Pd, Rh, and Ir are separated from Pt by a hydrolytic precipitation with $NaHCO_3$ under conditions of controlled acidity (pH 6–8) that leaves Pt, whose chloride complex undergoes no appreciable hydrolysis, alone in solution. The detailed analytical procedure appeared in the *Journal of the American Chemical Society* (32) and has been experimentally reevaluated (33; ref. 34, p. 67). It should be noted that researchers comparing hydrolytic methods with other methods are in effect examining their ability to use the method, which requires considerable experience and skill, especially with small samples (ref. 34, p. 75). Gilchrist also authored a number of review articles on the platinum metals in books, encyclopedias, and journals (9, 35–39).

Gold. In addition to his work on the platinum metals, Gilchrist was also considered an authority on the analytical chemistry of gold. In 1927 he investigated the effect of various stripping solutions on the assay of rolled gold plate (40), and by 1938 he had developed a new procedure for analyzing dental gold alloys, which involved the isolation of gold and base metals in the presence of nitrite ion, which complexes the platinum metals (41). This method, based on earlier work by Swanger (42), made use not only of the hydrolytic precipitation of base metals but also of the quantitative reduction of gold as a well-coagulated metal by NO_2^-, a reaction that can be used in refining processes to prepare gold of extremely high purity. With this procedure and the previous one for the platinum metals alone (7, 32), Gilchrist had solved the problem of analyzing complex platiniferous materials. The only problem remaining was lack of a suitable method for dissolving these refractory materials without contamination, a problem soon solved by Gilchrist's co-workers at NBS, Edward Wichers, Charles Lewis Gordon, and William George Schlecht (43, 44).

Miscellaneous. Gilchrist did not limit his research to the noble metals. He extended his work on controlled hydrolytic precipitation to various elements, for which he determined the alkalinity range within which their hydrated oxides could be quantitatively precipitated (45). He also suggested possible analytical separations based on careful control of pH. In a paper of 1960 (46) he devised a method for the separation of Ti, Zr, Fe, and Al from each other and their subsequent determination. He also developed methods for quantitatively separating and determining nonmetals, e.g., I^-, Br^-, and Cl^-. In a method involving controlled oxidation of halides (>1 mg of each ion present), he obtained standard deviations of 0.0002 g for the I^-, 0.0002 g for the Br^-,

and 0.0003 g for the Cl^- determinations (47).

Gilchrist devoted his later years to research on miscellaneous substances, especially the preparation of materials of high purity. To this related group of works belong his method for freeing Zr of common impurities and for preparing $Zr(SO_4)_2$ and ZrO_2 (48), preparation of high-purity $TiCl_4$ (49, 50) and determination of impurities in it by infrared spectroscopy (51), and the preparation of high-purity $NiCl_2$ (52), $BaTiO(C_2O_4)_2 \cdot 4H_2O$ (53), and sulfur (54).

At the beginning of Gilchrist's career, the state of development of the analytical chemistry of the platinum metals lagged far behind that of the other groups of metals, the methods then used being incomplete, inefficient, inaccurate, and inconvenient. Although in a number of cases, the methods developed by Gilchrist have been superseded by more modern methods, the fact that today the separation of the platinum metals is no longer shrouded in mystery is due in no small part to his pioneering efforts. Thanks to Raleigh Gilchrist and those who followed him, reliable procedures based on simple reactions are now available for the analysis of platiniferous materials—procedures comparable in accuracy with the best in use for more common metals.

Acknowledgments

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AAAS LAUNCHES PROGRAM FOR THE HANDICAPPED IN SCIENCE

The American Association for the Advancement of Science has officially launched its Project for the Handicapped in Science. The purpose of this initial project, which is funded by the Rehabilitation Services Administration of the Department of Health, Education, and Welfare through the George Washington University Rehabilitation Research and Training Center is to identify and explore barriers obstructing the entry and full participation of physically disabled persons to education and employment opportunities in science. Specifically, the project will seek to examine and evaluate ways in which the scientific professional associations and organizations of and for the handicapped can contribute to equal opportunities in science careers.

In order to build an ongoing and realistic program, the AAAS needs the expert consultation of handicapped individuals who have experienced difficulties in receiving an education to be a scientist or in professional placement becuase of their handicap. *If you are a disabled scientist*, please identify yourself to Martha Redden, Director, Project on the Handicapped in Science, Office of Opportunities in Science, AAAS, 1776 Massachusetts Avenue, N. W., Washington, D. C. 20036. The project will not use, without permission, the names of individual scientists who respond.

A Microbiological Study of Air and Surface Water Microlayers in the Open Ocean

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ABSTRACT

A 24-station trackline from Balboa, Panama to the Galapagos Islands and from the islands to Guayaquil, Ecuador comprised a microbiological study of water and air samples collected during an oceanographic research cruise aboard the R/V HAYES. Results showed that the total, viable, aerobic, heterotrophic bacterial populations decreased in direct relation with distance from land. The predominant bacterial isolates in water were marine species of the genus *Pseudomonas*, whereas the most commonly isolated bacteria from air samples were *Bacillus* spp., lending support to the terrestrial origin of microorganisms in the air over the open ocean.

Information presently available concerning the microbial flora of sea air and of the surface film layer of seawater is scanty, at best. The data which were available, in general, support the notion that the microorganisms found in air over the open oceans are of terrestrial origin (Gregory, 1961; Webb, 1961).

The aim of this study was to determine, at selected sites in the course of an oceanographic cruise aboard the U.S. Navy Oceanographic Vessel, the R/V HAYES, the bacterial populations of bulk surface water, air-sea interface, and air over the open ocean. The effective concentration of bacteria in the upper 30 μm (the microlayer) of the ocean was also measured and the types of bacteria isolated from samples collected at each location were identified and classified. The objective of the latter was to determine whether the bacteria found in air over the open ocean are of marine or terrestrial origin.

Materials and Methods

Samples were taken along tracklines running from Balboa, Panama to the Galapogos Islands and from the islands to Guayaquil, Ecuador during February, 1974 (Fig. 1).

Water samples were collected from a depth of 5 m using a Niskin sampler and from the top 600 μm with a Garrett-Sieburth screen sampler (Garrett, 1965). After collection, aliquots of the samples were subjected to Millipore filtration and plated onto Plate Count Agar (Difco) and SWYE [yeast extract (Difco), 0.3%; proteose peptone (Difco), 1.0%; NaCl, 2.4%; KCl, 0.07%; $MgSO_4 \cdot 7H_2O$, 0.7%; agar, 2.0%; pH 7.2 to 7.4] plates. Twelve 250-ml samples were taken from the

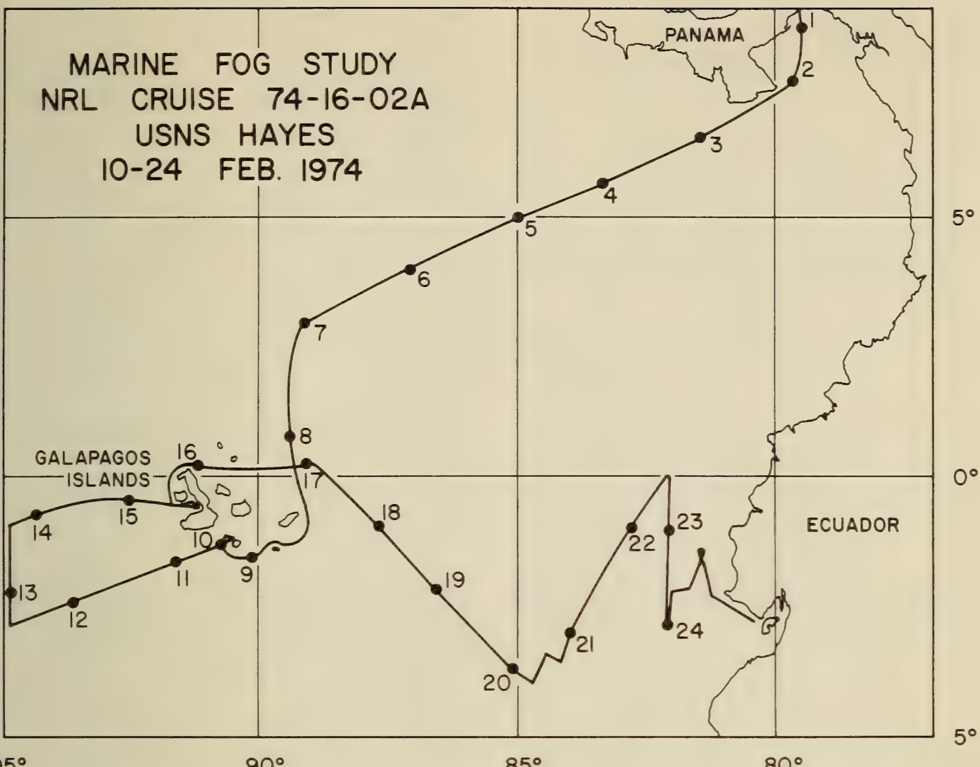

Fig. 1. Stations at which samples were collected during the 74-16-02A R/V HAYES cruise, February, 1974.

Niskin samples and 50-ml portions (a total of twelve) from the skimmer were used. All filtrations were done in duplicate.

Air samples were obtained from a height of 5 m with a NRL cascade impactor (Bressan, personal communication) run at 52 liters of air per minute for four consecutive ten-minute periods. These samples were streaked in duplicate onto PCA and SWYE plates. Qualitative air samples were collected with a modified Batelle impactor running at 2 liters/min of air for 8 hr. The material collected was streaked onto PCA and SWYE plates.

After inoculation, all plates were incubated at 22–23°C for 1 week, at which time they were enumerated. Subsequently the most abundant bacteria were picked and streaked for isolation onto either PCA or SWYE agar plates. Upon return to the laboratory, these isolates were purified and were maintained on ESWYE [yeast extract (Difco), 0.3%; proteose peptone (Difco), 0.3%; NaCl, 1.0%; KCl, 0.25%; $MgSO_4 \cdot 7H_2O$, 0.23%; agar, 2.0%; pH 7.2 to 7.4] slants.

The bacterial cultures were then identified according to a scheme based on the following characteristics: cell shape, Gram reaction (Bartholomew); motility at 16–24 hours; type of flagella present; catalase production; oxidase production; metabolic pathway for carbohydrate utilization (Hugh-Leifson test); sporulation; and fluorescence (Johnson and Colwell, 1974, Identification of aerobic heterotrophic bacteria. Manuscript in preparation.).

Results

The total number of aerobic, heterotrophic, viable bacteria in the water ranged from a high of 2.2×10^3/liter to a

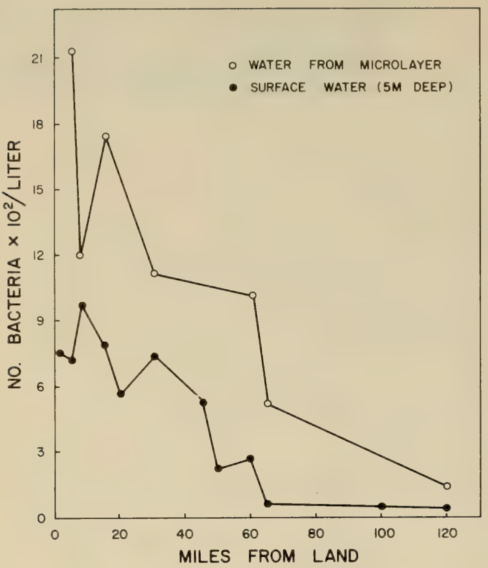

Fig. 2. Total counts at the stations at which samples were collected, with respect to distance from shore.

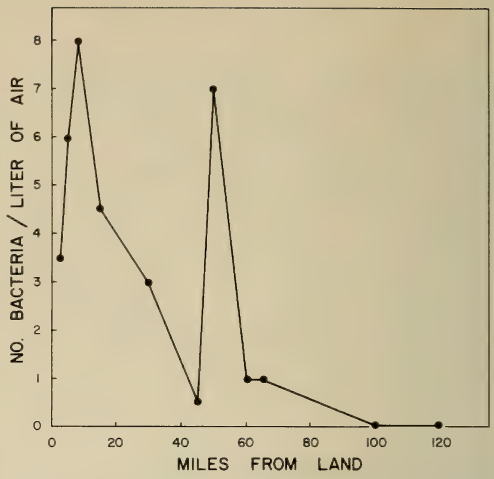

Fig. 4. Bacterial counts for air samples correlated with distance from land.

low of 20/liter and the total viable counts decreased as the distance from land increased (Fig. 2). Counts from microlayer samples were 1.3–6 times higher than those from bulk surface water samples (Fig. 3).

The bacterial population of the air ranged from 0 to 8/liter and also decreased as the distance from land increased (Fig. 4). It is interesting to note that the bacteria in the air were usually fewer than the molds (Fig. 5).

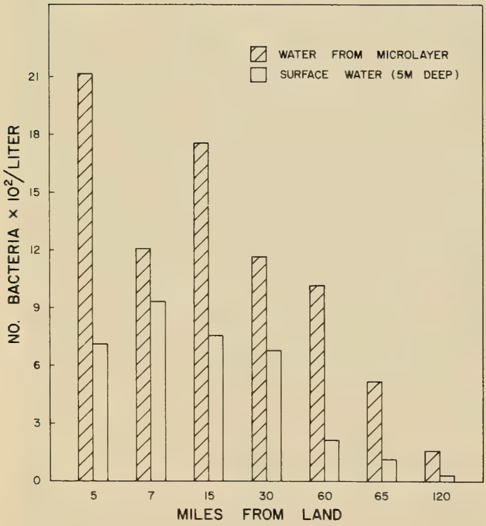

Fig. 3. Microlayer and bulk surface water sample bacterial counts.

The bacteria occurring most frequently in the water samples examined were *Pseudomonas* Type 3/*Spirillum* and *Pseudomonas* Type 2 spp., while species belonging to the genus *Bacillus* were most common in the air samples tested (Table 1).

Discussion

The results of the microbiological study carried out aboard the R/V HAYES (U.S. Navy Oceanographic Cruise No. 74-16-02A) support two established population trends and raise questions concerning the role of air-sea interaction insofar as the bacterial populations of the air are affected. The total, viable, aerobic, heterotrophic bacterial populations of both the water and the air decreased as the distance from land increased. This is in keeping with observations first made in the 1880's by Fischer (ZoBell, 1946) and Moultec (Gregory, 1961). In the case of aquatic bacteria, population size is believed to be primarily limited by nutrient concentrations (Wood, 1965), while humidity and exposure to ultraviolet light are more important factors in controlling the num-

ber of viable bacteria found in the air (Dimmick, 1969; Gregory, 1961; McDade and Hall, 1964; Webb, 1960).

The tendency of bacteria to collect at surfaces is reflected in the relative numbers of bacteria found at the air-sea interface and in water at 5m. The concentration of bacteria observed in the microlayer was 1.3–6 times greater than in the bulk water (Fig. 3), a concentration factor less than those reported for laboratory experiments on suspension of marine (Carlucci and Bezdek, 1972; Bezdek and Carlucci, 1974) and fresh-water (Blanchard and Syzdek, 1972) bacteria. This variation is due to differences in natural and artificial conditions and in the sampling techniques used. Under natural conditions, wind and capillary wave action cause continuous mixing of the microlayer with the bulk water, decreasing the concentration of bacteria at the sea surface. The decrease measured in the experiments carried out aboard the R/V HAYES was compounded by the fact that the screen sampler employed is a relatively inaccurate microlayer sampler (Garrett, 1965; Garrett, 1974; Hatcher and Parker, 1974; MacIntyre, 1974), in that it collects water from depths up to 600 μm. Thus, the sample becomes diluted and the inaccuracy arising there-

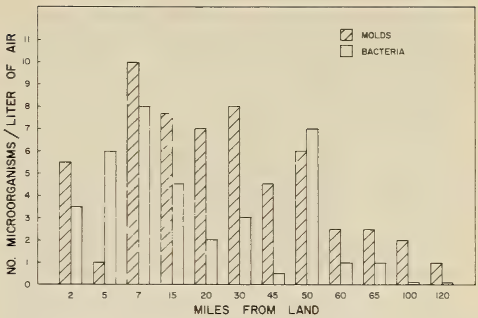

Fig. 5. Comparison of bacterial and mold counts for air samples.

Table 1.—Comparison of air and water samples.

Station	Miles from land	Number cultures examined	Predominant bacteria: Air	Predominant bacteria: Microlayer	Predominant bacteria: Water
1	0	3	*Vibrio/Aeromonas*		*Pseudomonas Type 3/Spirillum*
2	15	3	*Micrococcus*	*Pseudomonas Type 1*	*Pseudomonas Type 3/Spirillum*
3	45	3	*Micrococcus*		*Micrococcus*
4	120	6	*Bacillus*		*Acinetobacter*
5	100	3	*Bacillus*		*Vibrio/Aeromonas*
6	60	3	*Bacillus*	*Vibrio/Aeromonas*	*Pseudomonas Type 3/Spirillum*
7	20	3	*Streptococcus*		*Pseudomonas Type 3/Spirillum*
8	5	6	*Staphylococcus*	*Micrococcus*	*Pseudomonas Type 3/Spirillum*
9	2	3	*Bacillus*		*Pseudomonas Type 3/Spirillum*
10	7	3	*Bacillus*	*Micrococcus*	*Pseudomonas Type 3/Spirillum*
11	15	3	*Bacillus*	*Micrococcus*	*Pseudomonas Type 3/Spirillum*
12	65	6		*Pseudomonas Spirillum Type 3*	*Pseudomonas Type 3/Spirillum*
13	45	3			*Acinetobacter*
14	15	3	*Streptococcus*	*Vibrio/Aeromonas*	*Pseudomonas Type 3/Spirillum*
15	2	3			*Pseudomonas Type 3/Spirillum*
16	15	6	*Pseudomonas Spirillum Type 3*	*Pseudomonas Spirillum Type 3*	*Pseudomonas Type 3/Spirillum*
17	15	3	*Bacillus*		*Pseudomonas Type 2*
18	50	3	*Bacillus*	*Pseudomonas Type 2* (atypical)	
19	65	3	*Pseudomonas Spirillum Type 3*		
20	60	6	*Bacillus*	*Bacillus*	*Micrococcus*
21	30	3	*Bacillus*		*Bacillus*
22	15	3	*Bacillus*	*Bacillus*	*Pseudomonas Type 3*/(atypical)
23	15	3	*Bacillus*		*Pseudomonas Type 3*/(atypical)
24	15	6	*Bacillus*	*Bacillus*	*Pseudomonas Type 3*/(atypical)

from is magnified if one compares data obtained by this technique with results obtained using a bubble microtome in the laboratory (MacIntyre, 1968).

The main focus of this research was to compare the populations and types of bacteria found in air with those found in water and, thereby, to ascertain whether bacteria occurring in air over the open ocean are of terrestrial or marine origin.

That bacteria found in oceanic air are of terrestrial origin is based on the fact that the number of bacteria found in a given air mass decreases as the "age," that is, the number of hours from origination of the air mass from land areas, increases (Bressan, personal communication). Air mass age is calculated by measuring the amount of radon in the air. Radon, a gas with a half-life of three days and a product of the decay of uranium^{235} to lead, is given off into the atmosphere by land masses and not by the oceans. Thus, the age of an air mass can be determined by measuring the amounts of radon and its daughter products in the air (Larson, personal communication). Since the bacterial population decreases as the amount of radon decreases (and as the air mass age increases), it has been suggested that the bacteria found in air over the ocean are terrestrial in origin.

The belief that oceanic air bacteria are of marine origin is based on the role of bubbles in the air-sea interactions. Work done in the decade 1950–1960 revealed that most of the nonbiological particulate matter, i.e., ions and salt crystals, found in air over the ocean was ejected from the water by the breaking of bubbles (Woodcock and Blanchard, 1957). Subsequent laboratory experiments have shown that bubbles act as "scavengers" which collect and concentrate not only nonbiological particulates found in the water but also the bacteria present (Blanchard and Syzdek, 1972; Carlucci and Bezdek, 1972; Carlucci and Williams, 1965). When the bubble breaks, the attached material is ejected into the air. The importance of this phenomenon is evident when considered with the knowledge that, at any given instant, 3% of the ocean is covered with bubbles and that approximately 10^{18} of these bubbles break every second (MacIntyre, 1974). The number of bacteria introduced to the air by this mechanism must also be large.

These results, although not of a scope sufficient to be subjected to statistical analysis, suggest that bacteria found in oceanic air originate in both the marine and terrestrial environments. At 3 stations (16, 20, 22), bacteria found in the air were of the same generic group as those found in the water. At 2 other stations (0, 19), marine bacteria were isolated from the air (Table 1). These findings lend support to the bubble theory and the marine origin of oceanic air bacteria. However, it must also be pointed out that most of the bacteria found in water were members of the genus *Pseudomonas*, while the genus *Bacillus* was the most abundant type found in the air. *Pseudomonas* are predominant in the marine environment; *Bacillus* spp. are commonly isolated from soil and fresh water habitats. In addition, *Bacillus* spp. form spores, permitting survival when exposed to harsh environments. The presence of bacilli in the air samples suggests that these bacteria were carried out over the open ocean by the wind. The radon data, when analyses are completed, should permit observation and measurement of correlations between air mass age and generic composition of the bacterial populations and provide further elucidation of the "terrestrial origin" theory. Nevertheless, the data from this study indicate that both marine and terrestrial bacteria are found in air over the open ocean, with terrestrial types predominant.

Acknowledgments

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Beetles and Wasps Associated With *Cassia biflora* L. (Caesalpiniaceae) Fruits in Costa Rica, With a New Species of *Sennius* (Coleoptera: Bruchidae).

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ABSTRACT

Sennius biflorae is described as a new seed predator of *Cassia biflora* in Costa Rica, occurring in large numbers together with a dark, southern form of *S. auricomus*. Two other species, *S. celatus* and *S. fallax*, occur with *S. auricomus* in fruits of *C. biflora* in Mexico but, though present in Costa Rica, may be ecologically displaced from *C. biflora* there by *S. biflorae*. One record of *Acanthoscelides obrienorum* from *C. biflora* may represent an unusual or even spurious occurrence, as *C. skinneri* appears to be the normal host for this species in Costa Rica. Parasites are frequently numerous, with at least one species of braconid and three of chalcidoids as probable parasites of both *Sennius* species. The abundance and distinctiveness of the host plant, bruchids, and parasites make this plant-bruchid-parasite system a desirable one for detailed study.

In Mexico and Central America, some or all members of 6 bruchid genera are obligate seed "predators" (Janzen 1975: 157) of various species of *Cassia* L. All species of 3 genera (*Megasennius* Whitehead and Kingsolver, *Pygiopachymerus* Pic, and *Sennius* Bridwell) attack *Cassia*, whereas only 1 or a few species of the other 3 (*Acanthoscelides* Schilsky, *Amblycerus* Thunberg, and *Zabrotes* Horn) do. Some species of other genera may feed on *Cassia* seeds but are not restricted to *Cassia*; *Stator limbatus* (Horn) is an example. Genera and species specializing on the "Cassia" section of *Cassia*, including *Megasennius*, *Pygiopachymerus*, and 1 species of *Zabrotes*, were discussed by Janzen (1971) and Whitehead and Kingsolver (1975). Various species in other sections of *Cassia* are attacked by several species of *Amblycerus*, 1 species of *Acanthoscelides*, all species of *Sennius*, and 1 recently discovered, undescribed species of *Zabrotes*. At this time, there is no clear correlation between seed predator species and these other sections of *Cassia*.

The genus *Sennius* was reviewed recently by Johnson and Kingsolver (1973). This treatment has so far proven adequate for most of the Costa Rican fauna, but we have found that 2 species commonly reared from *Cassia biflora* L. require discussion in order to update the revision of *Sennius*; 1 is new, and the other a geographic variation. Whitehead (1975) prepared a preliminary list of parasitic wasps associated with bruchid-infested fruits in Costa Rica, and more specific data for those associated with *C. biflora* are included herein.

To facilitate comparisons, the description of the new *Sennius* follows the format used by Johnson and Kingsolver; genital figures are included, but habitus figures are excluded as they would be of little help in identification. Specimens are deposited in the Northern Arizona University, Flagstaff (NAUF) and U. S. National Museum of Natural History (USNM); these were obtained from rear-

ings made by D. H. Janzen and from examination of pertinent materials in the National Herbarium. We thank Janzen also for reading the manuscript and for partial support from NSF Grants GB 35032X and BMS 75-14268.

Sennius auricomus Johnson and Kingsolver

This was described as a pale species ranging from Mexico to Venezuela (Johnson and Kingsolver 1973), but specimens from Costa Rica are in general much darker than are Mexican specimens; only 1 specimen was found with the pale pygidium and abdomen characteristic of northern specimens, and only a few have pale elytral markings. Genital characters, however, match those of the pale northern form. This species is known only from *Cassia biflora*, and new records are the following.

COSTA RICA. Guanacaste: Bebedero, Taboga, 1930, O. Jimenez #777, from herbarium specimen of *C. biflora*; Bebedero, Taboga, 1.III.1972, D. H. Janzen #20–42, reared from *C. biflora*; Cañas, 3.III.1972, D. H. Janzen #20–5, reared from *C. biflora*; Cañas, D. H. Janzen #1972-001, reared from *C. biflora*.

Sennius biflorae Whitehead and Kingsolver, **new species**

Description.—Length (pronotum-elytra) 1.5-2.1 mm. Width 1.0-1.6 mm. Maximum thoracic depth 0.8-1.2 mm.

Integument black except as follows: Head without postocular spot; antenna rufotestaceous, unicolorous; labrum rufous; labium and palpi rufotestaceous. Front coxa dark rufous, leg otherwise rufotestaceous. Middle coxa black, leg otherwise rufotestaceous. Hind coxa black, trochanter rufous, leg otherwise rufotestaceous. Elytron varied from wholly black in most small specimens, through having small inconspicuous spot in basal ⅓, to having large circular rufous spot across intervals 3–10.

Vestiture sparse, whitish or yellowish. Dense white in small areas behind postocular lobe, on scutellum, and along posterior margin of metepisternum. Pygidium with vague pattern of dense white vestiture basally, especially along midline.

Head short and broad, densely punctulate; frons with median carina low, narrow, alutaceous, extended from frontoclypeal suture to vertex; frons width about equal to width of eye; ocular sinus about ¾ as long as width of eye; postocular lobe short; distance from base of antenna to apex of labrum about ½ as long as distance from upper limit of eye to apex of labrum; antenna short, article 1 filiform, 2 and 4 moniliform, 3 subfiliform, 4 shorter than adjacent articles, 5–10 eccentric and transverse, 11 about as long as wide and subacute apically; antenna extended to about base of pronotum.

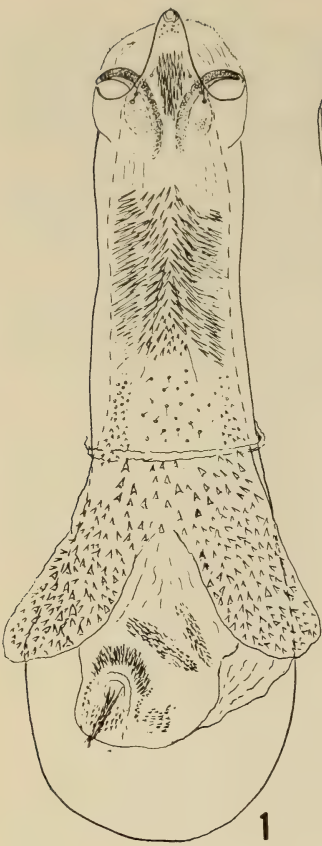
Pronotum with disc subcampanulate, coarsely punctate; lateral carina strong from base ½ way to coxal cavity; shallow median impression from basal lobe to basal ¼, impunctate basally. Procoxae separated by prosternum except at apices.

Pterothorax with scutellum transverse, bidentate, with dense vestiture. Elytron slightly less than twice as long as broad, dorsal surface evenly convex between humerus and median margin; striae deep, finely punctate, no distinct mucronations basally; intervals finely punctulate; striae 5 and 6 closer to one another at base, striae otherwise subequally spaced; humerus not differentiated in sculpture. Venter finely punctulate. Hind coxa punctate. Hind femur clavate; ventral surface flat, with fine inner carina, subapical acuminate spine about ½ as long as width of tibial base. Hind tibia with ventral, lateral, and dorsomesal glabrous longitudinal carinae, lateroventral carina strongly developed in basal ⅔; tibial corona with 2 or 3 dorsal spinules, lateral tooth moderate, mucro slightly longer than lateral tooth, sinus at base of mucro shallow. First tarsomere with ventral, lateral, and mesal glabrous longitudinal carinae.

Abdomen with sternum 1 not flattened medially, about as long as remaining sterna, posterior margin straight; sterna 2-4 unmodified; sternum 5 emarginate in male, entire in female; pygidium punctate, convex in lateral view.

Male genitalia (Figs. 1–2). Median lobe moderately long; ventral valve slender, lateral margins concave in ventral view, base much narrower than apex of median lobe, arcuate in lateral view; hinge sclerites small, falcate; internal sac with elongate spine cluster above ventral valve and densely spinulate mass from near apices of hinge sclerites to middle of sac, apical ½ of sac and diverticula lined with fine triangular spicules. Lateral lobes elongate, slightly bowed in ventral view, cleft for more than ¾ their length, apices expanded mesally and setose.

Type series.—Holotype male, "COSTA RICA. Guanacaste Prov. 1.5 mi. W. Canas 3.III.1972 D. H. Janzen #20-V" and "Reared from Cassia biflora emerged by 20.VI.1972"; type no. 73568, in U. S. National Museum of Natural History. Allotype female and 50 paratypes, same data, in NAUF and




Figs. 1–2, *Sennius biflorae*; male genitalia: 1, median lobe; 2, lateral lobes.

USNM. Another 32 paratypes in USNM have the following data.

COSTA RICA. Guanacaste: Bagaces, 11.II.1971, D. H. Janzen 453, reared from *C. biflora*; Bebedero, Taboga, 1930, O. Jimenez #777, from herbarium specimen of *C. biflora*; Bebedero, Taboga, 1.III.1972, D. H. Janzen #20–42, reared from *C. biflora*; Playa de Coco (Playa Panama), 14.III.1971, D. H. Janzen #617, reared from *C. biflora*.

About 590 additional, poorly preserved, unmounted specimens have the following data.

COSTA RICA. Guanacaste: Cañas (Finca La Pacifica), D. H. Janzen #1972-001, reared from *C. biflora*.

Discussion.—Named for the host plant, *Cassia biflora* L., *Sennius biflorae* is a doubly apt name since most specimens are characterized by a red spot on each elytron. In material reared by Janzen and in herbarium material collected by Jimenez, *S. biflorae* was accompanied by *S. auricomus* but was generally more numerous.

Sennius biflorae, despite being very differently colored, seems most closely related to *S. atripectus* Johnson and Kingsolver because of genital similarities and is thus placed in the Fallax Group; *S. atripectus* differs from *S. biflorae* by having much of the body red orange, minute spines at bases of elytral striae 2-6, and a much shorter ventral valve. Specimens of *S. biflorae* key to couplet 27 and nearest to *S. auricomus* in Johnson and Kingsolver (1973) but are distinguished by having sparse vestiture and by lacking the distinctive spine clusters of the endophallus characteristic of *S. auricomus* (Johnson and Kingsolver 1973: Fig. 23); the dense vestiture of *S. auricomus* contributes to a pale, ochraceous appearance, whereas *S. biflorae* is black in appearance and normally spotted.

Sennius medialis (Sharp) and *S. biflorae* resemble one another but are not closely related. Among the numerous differences are the following for *S. medialis*. Elytral maculation: red spot larger in nearly all specimens. Vestiture: dense basal vestiture on intervals 3 and 5; pygidial vestiture long. Head: proportionately long. Pronotum: midline punctures very fine, few slightly larger punctures (not mostly large and shallow). Elytron: striae at base well defined; intervals with cross striations regular. Male genitalia: internal sac not strongly trilobed; spicules in apical ½ of internal sac more slender; ventral valve broad.

Acanthoscelides obrienorum Johnson

We recently received from John Silander, Duke University, a small sample of bruchids including several specimens of *S. biflorae* and 1 of *A. obrienorum* reared from *C. biflora* in "Guanacaste Province," Costa Rica in 1973. This is our first record of *A.*

obrienorum associated with *C. biflora* in Costa Rica, although C. D. Johnson (pers. comm.) has Mexican records of this association.

In Mexico, *A. obrienorum* is known to attack members of several sections of *Cassia* (e.g., "Gaumerocassia,", "Palmerocassia," and "Pterocassia"). In Guanacaste Province, Costa Rica, however, it has been reared repeatedly from *C. skinneri* Benth. ("Phragmocassia" section) but except for this record from *C. biflora* ("Peiranisia" section) has no other known host association. Consequently, we suspect that *A. obrienorum* normally plays no major role in the *C. biflora* fauna in Costa Rica.

Parasitic Hymenoptera

Three of Janzen's reared samples from *Cassia biflora* included various parasitic wasps; these are treated here according to Whitehead (1975).

Janzen #20-5 and #1972-001: *Heterospilus* sp. #2 (Braconidae), *Horismenus* sp. nr. *missouriensis* (Ashmead) (Eulophidae), *Eupelmus* sp. nr. *peruvianus* (Crawford) (Eupelmidae), and ?genus sp. #3 (Pteromalidae).

Janzen #20-42: *Heterospilus* sp. #2 and *Chelonus* sp. (Braconidae).

Except for the moth parasite *Chelonus*, these presumably are bruchid parasites and probably parasitize both of the *Sennius* species. Parasite numbers are large in some samples, with *Horismenus* represented by hundreds of individuals in samples #20-5 and #1972-001, but parasites are completely absent in other samples. Several other parasites may be expected, notably the braconid *Stenocorse bruchivora* (Crawford).

Discussion

Johnson and Kingsolver (1973) reported a reared series of *S. auricomus*, *S. celatus* (Sharp), and *S. fallax* (Boheman) from *C. biflora* in Nayarit, Mexico. *Sennius celatus* and *S. fallax* occur in Costa Rica, attack a wide range of *Cassia* species, and may therefore be expected occasionally in *C. biflora* in Costa Rica. However, it seems likely that they have been ecologically displaced by *S. biflorae* from that host and are not an important part of the *C. biflora* fauna in Costa Rica. It may also be that Costa Rican *S. celatus* and *S. fallax* differ from their northern forms and are not adapted to this particular host in Costa Rica; comparative preferences studies may, therefore, be of considerable interest and may yield data useful for analysis of geographic variation.

Except for *S. discolor* (Horn), species of the Fallax Group probably all occur in or are restricted to seeds of members of the "Peiranisia" section of *Cassia*. Apparently, however, *S. auricomus* and *S. biflorae* specialize on just 1 member of this section, whereas other members of the Fallax Group attack several host species.

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Colaspis melancholica Jacoby and Its Close Relatives (Coleoptera: Chrysomelidae)

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ABSTRACT

The taxonomy of *Colaspis melancholica* and its close relatives is discussed. *Cholaspis spinigera*, *C. diduma*, *C. guatamalensis*, *C. shuteae*, and *C. brownsvillensis* are described as new species, and *C. balyi* and *C. nigrocyanea* are also discussed. A key to the seven species is presented.

The genus *Colaspis* for the most part consists of groups of species. These groups are easily recognized, but the species within each of the groups are so much alike that, without dissecting for the aedeagus, one has great difficulty in naming them. For example, the yellow brown costate species of *Colaspis* in the United States were for years regarded as one species. Horn wrote that *C. brunnea* "is an insoluble complex." The present group, which I call the *melancholica* group, is quite unlike the costate group, being without costae and black in color. However, it resembles the *brunnea* group in being composed of species so alike that even I, who have been studying them for some time, am unable to recognize them without dissecting for the aedeagus, and I cannot name the females at all with any certainty. In addition to the aedeagus the male has another important feature—a spinelike projection near the end of the hind tibiae—found in 2 of the 8 species of the group. The others have a slight swelling on the hind tibiae instead of the spine found in some of the *brunnea* group.

Colaspis melancholica Jacoby
Figs. 1 & 2

Colaspis melancholica Jacoby, Biol. Centr.-Amer. Col., Vol. VI, 1881, p. 143.

Length 6.8 mm. Width 3.5 mm. Elongate oval, shining black with metallic green glints in interior of punctures and a green lustre on legs and under-surface. Densely punctate throughout.

Head with interocular space a little more than half width of head, densely punctate over front, punctures obscuring frontal tubercles, as well as

Key to Species of *Colaspis*

1. Hind tibiae in male with spinelike projection near end of hind tibiae 2
   Hind tibiae in male without spinelike projection 3

2. Aedeagus with a long, pointed asymmetrical tip .... *melancholica* Jacoby
   Aedeagus with a pointed, shorter and not asymmetric tip .... *spinigera*, n. sp.

3. Head with interocular space half width of head 4
   Head with interocular space more than half width of head 5

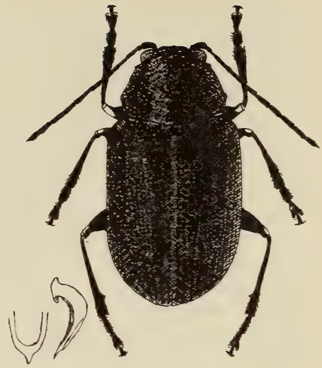
4. Aedeagus with a very short tip .... *balyi Jacoby*
   Aedeagus with a longer tip .... *diduma*, n. sp.

5. Elytra not quite 3 times as long as prothorax .... *nigrocyanea* Crotch
   Elytra 3 times as long as prothorax 6

6. Aedeagus broad before tip with a pointed tip .... *guatemalensis*, n. sp.
   Aedeagus narrowing more gradually before tip .... *brownsvillensis*, n. sp.

1. Colaspis melancholica Jac.

2. C. melancholica Jac.



3. C. spinigera n.sp.

4. C. diduma n.sp.

5. C. balyi Jac.

6. C. shuteae n.sp.

7. C. brownsvillensis n.sp.

8. C. nigrocyanea Crotch

9. C. guatemalensis n.sp.

clypeus boundaries, labrum and mouthparts dark. Antennae black except joints 3–5 which are pale beneath. Prothorax about one-third wider than long with sides having an angularity below middle, rather irregularly punctate with some small flat impunctate spaces. Scutellum shining black with faint green lustre. Elytra not quite 3 times as long as prothorax and wider, densely covered with punctures in irregular lines, near suture and apex in single rows, elsewhere tending to be geminate with faintly costate ridging between, more pronounced on sides and at apex, epipleura as well as punctures with metallic green lustre. Body beneath with prosternum punctate, legs and ventral surface dark, shining with metallic lustre. A spinelike projection on male near apex of hind tibia.

Type.—Male, in British Museum (Natural History).

Type locality.—Tuxtla, Mexico, Salle collection (labelled *syntype*).

Other locality.—Jicaltepec, Vera Cruz, Mexico.

Remarks.—In the material labelled *Colaspis melancholica* from the British Museum (Natural History) there are at least 2 species in addition to *C. balyi*, which Jacoby first described as a distinct species and later as a variety of *melancholica*. It is quite distinct from *C. melancholica*, as shown by the aedeagus and its general shape. I have chosen from the syntypes sent me labelled *melancholica* a specimen from Tuxtla, Mexico, as the type of *melancholica*. In the male the small spinelike projection on the hind tibiae can be used to distinguish this species from most of the others of the *melancholica* group. Another species bearing the syntype label is from El Reposo. Besides lacking the spine like projection on the hind tibiae, this species may be separated by the shape of the aedeagus. In the Tuxtla specimen the aedeagus has a long asymmetrical tip, while in the El Reposo specimen the tip is shorter and not asymmetrical. At first I thought the asymmetrical tip of *melancholica* was an accident, but when I had dissected the second specimen which had the same asymmetry I realized that it was just another species with an asymmetric tip that occurs in several species of the genus *Colaspis*.

Colaspis spinigera, n. sp.

Fig. 3

Length 5 mm. Width 3 mm. Elongate oval, shining black with inside of punctures with a blue green lustre, labrum and antennae dark, densely punctate above, mesosternum also punctate.

Head with interocular space more than half width of head, punctures on top of head fine and inconspicuous becoming coarser below, frontal tubercles alone smooth. Antennae entirely dark. Prothorax not twice as wide as long with coarse punctures irregularly irregularly arranged with a few smooth spots, sides with angularity below middle. Scutellum shining, smooth, black. Elytra densely punctate throughout, in single lines near suture and apex, elsewhere irregularly geminate. Body beneath and legs entirely dark, shining with a bluish lustre, hind legs in male with a short, spinelike projection near apex.

Type.—Male, Museum of Comparative Zoology.

Type locality.—Cuernavaca, Morelos, Mexico, Wickham collection.

Remarks.—This species resembles *melancholica* in having the hind tibia with a projection like a spine, but the aedeagus does not have as long a pointed tip and is not asymmetrical. However, the tip is longer than in most of the group.

Colaspis guatemalensis, n. sp.

Fig. 9

Length 5.7 mm. Width 3.2 mm. Ovate, shining black with metallic green lustre in middle of punctures. Upper surface densely punctate.

Head with interocular space more than half width of head, front so densely punctate as to obscure frontal tubercles and boundary of clypeus. Antennae with joints 2-6 pale and joints 8 and 9 a little darker, remainder entirely black. Prothorax nearly twice as wide as long with scattered punctures, sides angulate below middle. Scutellum shining black. Elytra 3 times as long as prothorax and a little wider with punctures in single line near suture and apex and irregularly geminate elsewhere with metallic green lustre inside punctures. Body beneath dark, prosternum punctate, legs dark, hind tibia slightly widened near apex.

Type.—Male in British Museum (Natural History).

Type locality.—El Reposo, 800 ft., Guatemala, Champion collector.

Other locality.—Guatemala City, E. G. Smyth.

Remarks.—The shape of this species is different from that of *melancholica* in having a broader and shorter prothorax with the elytra fully 3 times as long as the prothorax. The elytra are not so densely punctate as in *C. melancholica*, and the aedeagus has a shorter narrow tip.

Colaspis balyi Jacoby
Fig. 5

Colaspis balyi Jacoby, Biol. Centr. Amer. Vol. VI, pt. 1, 1881, pp. 143–4.
Colaspis melancholica var. *balyi* Jacoby, Biol. Centr. Amer. Vol. VI, Suppl. Nov. 1890, p. 222.

Length 5.2 mm. Width 2.5 mm. Elongate oval, shining black, densely punctate above.

Head with interocular space approximately half width of head, densely punctate except over frontal tubercles which are smooth, labrum and mouthparts dark. Antennae almost entirely black, only a little paler in apical half of second and fourth joints. Prothorax a little wider than long with sides subangulate below middle, disc densely punctate. Scutellum black. Elytra 3 times as long as prothorax and a little wider, densely punctate, punctures near suture and apex in single lines, irregularly geminate elsewhere. Surface with cross ridging and on sides and near apex with some semi-costate ridging. Body beneath and legs black, prosternum punctate.

Type.—Male in British Museum (Natural History).

Type locality.—Duenas, Guatemala, Champion collector.

Remarks.—Although this species undoubtedly belongs in the same group as those from Tuxtla and El Reposo, it is quite unlike either of them. It is smaller in size and of narrower shape. Also, the space between the eyes is only half the width of the head. The aedeagus has an even shorter, narrower tip than in either of the preceding species, and there is not a sign of metallic coloring either in the punctures or on the body beneath.

Colaspis nigrocyanea Crotch
Fig. 8

Colaspis nigrocyanea Crotch, Proc. Acad. Phil. Vol. XXV, 1873, p. 45; Horn, Trans. Am. Ent. Soc., Vol. XIX, 1892, pp. 223, 224.

Length 5.5-5.8 mm. Width 3-3.3 mm. Oval, shining dark brownish black or black, densely punctate above.

Head with interocular space considerably more than half width of head, front of head densely punctate except the smooth frontal tubercles, labrum and mandibles reddish brown. Antennae with basal 5 joints reddish brown, rest dark. Prothorax not twice as wide as long with sides angulate below middle, disc varying in punctures, some being densely punctate, others, in having punctures scattered with smooth areas between. Scutellum shining black. Elytra 3 times as long as prothorax and wider, punctures near suture and apex in single lines, elsewhere irregularly geminate. Body beneath and legs dark reddish brown or black, prosternum punctate.

Type.—Whereabouts unknown.

Type locality.—Arizona.

Other localities.—Arizona: Tucson, Pimala.

Remarks.—Some of the specimens lack the metallic blue or green in the punctures or undersurface from which Crotch derived the name *nigrocyanea*. The aedeagus is much like that of *C. brownsvillensis*, the species from Texas, but the short tip is very narrow.

Colaspis brownsvillensis, n. sp.
Fig. 7

Length 6-6.5 mm. Width 3-3.5 mm. Ovate shining black all over except the second, third, fourth and fifth antennal joints which are a little paler brown; densely punctate throughout the upper surface and sides of prosternum.

Head with interocular space a little more than half width of head, densely punctate, punctures obscuring boundary of clypeus. Antennae dark with joints 6-11 deep black. Prothorax nearly twice as wide as long in one of the specimens, not so wide in some others, with scattered punctures in middle of disc, and denser punctation on sides, margin somewhat angulate. Scutellum shining black. Elytra 3 times as long as prothorax and wider, densely and somewhat irregularly punctate, punctures along suture and apex in single lines, elsewhere irregularly geminate. Body beneath entirely dark, sides of prosternum punctate, legs dark.

Type.—Male and 1 male peratype, 1 female paratype.

Type locality.—Brownsville, Texas, Wickham collector, 1913.

Remarks.—This is a close relative of *C. nigrocyanea* Crotch, but the eyes are closer together, the elytra are not

so densely punctate, and in general the beetles are a little larger than in *nigrocyanea*.

Colaspis shuteae, n. sp.
Fig. 6

Length 4.8 mm. Width 2.6 mm. Oval, shining black with inside of punctures metallic green, densely punctate above.

Head with interocular space a little more than half width of head, a small median depression on front, punctures fine and dense covering all but frontal tubercles, labrum dark reddish brown. Antennae with joints 2 to 6 paler, remainder dark. Prothorax not quite twice as wide as long with a slight angularity on sides below middle, also with large punctures irregularly arranged, leaving middle bare in spots. Scutellum shining black. Elytra 3 times as long as prothorax and a little wider with punctures in single lines near suture, apex and along sides, irregularly geminate elsewhere with some raised edges forming slight costae, more pronounced at apex and on sides, also transversely raised across middle. Body beneath and legs dark, prosternum punctate.

Type.—Male, in British Museum (Natural History).

Type locality.—Rio Frio, Colombia, George Salt, June 26, 1927.

Remarks.—The aedeagus of this species is similar to that of *C. balyi* but with a longer point. I am naming this after Mrs. Sharon Shute of the British Museum (Natural History), who has been of great help in picking out specimens for study and who has dissected many of them for me.

Colaspis diduma, n. sp.
Fig. 4

Length 4.8 mm. Width 2.6 mm. Brownish black with deep brownish black legs and undersurface. Upper surface with many punctures not as dense on elytra as on prothorax.

Head with interocular space approximately half as wide as head, a median depression on front, densely punctate throughout with only frontal tubercles smooth. Antennae missing. Prothorax not twice as wide as long with sides having angularity below middle, densely punctate. Scutellum dark brown. Elytra 3 times as long as prothorax and a little wider. Punctures in single lines near suture and at apex, irregularly geminate elsewhere. Body beneath with punctures on prosternum. Legs and abdomen dark reddish brown.

Type.—Male in Museum of Comparative Zoology.

Type locality.—Cochabamba, Bolivia.

Remarks.—In outline this species resembles *C. shuteae*, which was collected in Colombia, but the beetle is dark brown, almost black, not very shiny, with no metallic green lustre at all, and the punctures are not so dense or large on the elytra. The elytra do not have the rough surface of the rest of the *melancholica* group. The tip of the aedeagus is longer than in most of the group but not as long as in *C. melancholica* and not at all asymmetrical.

BOARD OF MANAGERS MEETING NOTES

April 29, 1975

The 629th meeting was called to order at 8:05 p.m. by President Stern in the Conference Room in the Lee Building at FASEB. President Stern introduced Dr. Henry Liers who presented a proposal for the Bicentennial celebration entitled "Science, Engineering and Society." A lively discussion followed with consensus that the proposed divisional structure for the Academy would certainly assist in the implementation of the program. The minutes of the previous meeting were corrected.

Treasurer.—Dr. Rupp's report was optimistic. With $2000 cash in hand and dues still coming in, the Academy can look forward to another year's operation in the black.

Membership.—Dr. Florence Forziati presented eight nominees for fellowship: Dr. Raynor L. Duncombe, Dr. Nicolae Filipescu, Dr. Arthur Jensen, Dr. Howard St. Claire Jones, Dr. Milton N. Kabler, Dr. William V. Loebenstein, Dr. James F. Goff, and Dr. Joseph M. Marchello. (See Vol. 65, No. 3, JWAS. —Ed.) Two new delegates were made Fellows: Dr. John O'Hare, representing the new affiliated society, The D.C. Psychological Assoc.; and Dr. Carl H. Cotterill, representing the American Institute of Mining, Metallurgical and Petroleum Engineers. A motion for acceptance of these new fellows was seconded and approved unanimously by the Board.

Policy Planning/Ways and Means.—Dr. Alphonse Forziati presented the Committee's recommendations for the three special tasks assigned to it by the Board at the February 11 meeting:

1. Method of counting ballots: The committee recommended adoption of the simple plurality system rather than the Hare system. A motion to this effect was made by Dr. Sulzbacher, seconded by Dr. Honig, and passed unanimously by the Board.

2. Achievement Award in the Behavioral Sciences: The Committee's recommendation for the establishment of an achievement award in the Behavioral Sciences evoked a discussion concerning the intention that the award be for first-hand observational laboratory studies—i.e., original field studies. A motion for approval by Dr. Jean Boek was seconded by Dr. Sulzbacher and approved unanimously by the Board.

3. Divisional Structure: "In view of the diverse areas of interest to Academy affiliates, the committee recommends the adoption of a divisional structure." A motion for the adoption of the divisional structure was made by Dr. Rupp and seconded by Dr. Franz. A lengthy discussion followed with the vote on the original motion being nine for and eight against.

Dr. Abraham, President-elect, then requested that everyone interested in working in an advising capacity contact him.

Dr. Honig, on behalf of the Academy, extended a vote of thanks and congratulations to the committee for an excellent job.

Encouragement of Science Talent.—Mrs. Shafrin announced that, at the Awards banquet to be held on May 19 in

conjunction with the Joint Board on Science and Engineering Education, awards were to be given to 40 outstanding students.

Mrs. Shafrin proposed that a Memorial be established for Berenice Lamberton, who had been an outstanding teacher and an advisor for the encouragement of science in the Junior Academy for many years.

Dr. Cook made a motion, seconded by Mr. Sherlin, that the Teaching of Science Award for secondary Teachers be called the *Berenice Lamberton* Award.

Mrs. Shafrin made a motion that a *Berenice Lamberton* award plaque be awarded to the first-prize winner of the D.C. Science Fair each year, the cost to be funded by the Senior Academy. An amendment to the motion—that the plaque go to the school for display and that a certificate be given to the student—was passed.

New and Old Business.—The first issue of the symposium "Energy Recovery From Solid Wastes" is being edited by Dr. Harvey Alter and Dr. Richard H. Foote. (To appear as the March, 1976 issue of the *Journal WAS*.—Ed.)

A motion by Dr. Honig, seconded by Dr. Abraham, that a letter of commendation be sent to Dr. William Zisman upon his retirement from the Naval Research Laboratory was approved unanimously. Dr. Abraham and Mrs. Shafrin will prepare the commendation.

Dr. Forziati presented a proposal from the Washington Paint Technical Group for affiliation with the Academy. The Policy Planning/Ways and Means Committee recommended that a letter be sent to the Group advising them that their application was still under consideration.

The committee also recommended that the Academy Bylaws (Article II, Section 6) he amended to delete the words "who has been nominated as a delegate by a local affiliated society" so that Section 6 would apply only to Awardees. The amended form of Section 6 would simplify the affiliation of groups such as the Washington Paint Technical Group.

Dr. Honig made a motion approving transmittal of the letter to the Washington Paint Technical Group but proposed that the newly elected Board of Managers take up the question of amending the Bylaws. This motion was seconded and approved by the Board.

President Stern presented a letter from Edward D. Andrus, Manager of the Range Facilities Department, regarding the establishment of a "Grant-in-Aid" project to a graduate student which would deal with the problem of adequately ventilating an indoor shooting range. The Board was asked to submit suggestions to Dr. Stern.—Mary H. Aldridge, *Secretary*.

(*Editor's note: The minutes appearing above have not yet been read or corrected. They are published now in the interests of keeping the membership of the Academy up to date. Any amendments will be published in a future issue.*)

NEW FELLOWS

Robert F. Brady, Chief, Paints Branch, General Services Adm., Washington, DC., in recognition of his contributions to organic chemistry, and in particular his syntheses and characterization of ketoses critical in the diagnosis of pentosuria. *Sponsors*: A. F. Forziati, Philip J. Franklin.

Meryl N. Christiansen, Chief, Plant Stress Lab., Agricultural Res. Ctr., USDA, Beltsville, Md., in recognition of his contributions to the knowledge of cottonseed quality, biochemistry, and germination of an understanding of chilling injury to crop plants, as well as for his innovative leadership covering broad

aspects of plant stress research. *Sponsors*: J. L. Hilton, Patricia Sarvella.

Ralph I. Cole, formerly American Univ., retired, in recognition of his contributions to engineering systems, management of research and development, and engineering education. *Sponsors*: George Abraham, Henry Liers, E. L. Brancato.

John W. Lyons, Director, Center for Fire Research, National Bureau of Standards, in recognition of his contributions to the understanding of polyelectrolytes, applied rheology, fire retardants, and fire research at NBS. *Sponsors*: George Abraham, Richard K. Cook, Grover C. Sherlin.

Mark B. Mendelsohn, Ass't Prof., Dept. of Psychology, George Mason Univ., Fairfax, Va., in recognition of his contribution to the field of clinical psychology, particularly his current research upon the effects of behavioral reduction of disruptive behavior upon the level of serotonin in the brain, and to the scientific community of Washington by initiating the development of a Behavioral Sciences Branch as part of the WAS. *Sponsors*: Kurt H. Stern, Kelso B. Morris.

Flora G. Pollack, Mycologist, APHIS, USDA, Beltsville, Md., in recognition of her contributions to mycology, and in particular her systematics research on economically important Fungi 'Imperfecti' from all parts of the world. *Sponsors*: Richard H. Foote, John A. Stevenson, R. R. Colwell.

Rafael Sarmiento, Research Chemist, Agricultural Research Center, ARS, USDA, Beltsville, Md., in recognition of his contribution to the development of microanalytical techniques for and the synthesis of pest control agents such as repellents, attractants, juvenile hormone mimics and herbicides. *Sponsors*: R. J. Argauer, Mary H. Aldridge.

SCIENTISTS IN THE NEWS

FOOD AND DRUG ADMINISTRATION

Edward O. Haenni, Consultant, Bureau of Foods, was reappointed for a second four-year term as Chairman, Commission on Food Additives, Applied Chemistry Division, IUPAC, at the recent 28th IUPAC Conference in Madrid. As a result of a restructuring of the Division at the Madrid meeting, the Commission has increased responsibility, reporting directly to the Division instead of to the Food Section, which was abolished.

Dr. Haenni was also reappointed this year as a member of the Subcommittee on Specifications, Food Chemicals Codex, NAS-NRC. He continues to serve on the Centennial Subcommittee on Mementos, American Chemical Society, and as Chairman of the Centennial Committee of the Chemical Society of Washington.

At its annual October meeting, the Association of Official Analytical Chemists awarded Dr. Haenni a certificate as a Fellow of the Association.

FASEB

George W. Irving, Jr., ASBC member and Research Associate in FASEB's Life Sciences Research Office, was honored by the American Chemical Association at its recent Fall meeting in Chicago. Dr. Irving received the John R. Kuebler Award, the highest honor of Alpha Chi Sigma, the professional chemical fraternity, for a "distinguished career of service to the fraternity, the scientific community, the government and the general public." Formerly Administrator of the Agricultural Research Service, USDA, Dr. Irving joined the LSRO staff in 1972 where he coordinates the review of the health aspects of GRAS substances, currently a major effort of LSRO for FDA. He has an interesting article on the GRAS list in the April 1975

issue of FEDERATION PROCEEDINGS.

UNIVERSITY OF FLORIDA

Dr. R. I. Sailer, formerly Chairman of the Insect Identification and Beneficial Insect Introduction Institute at the Beltsville Agricultural Research Center, USDA, has been elected President-Elect of the Entomological Society of America.

OBITUARY

Aaron L. Shalowitz

Aaron L. Shalowitz, 82, an engineer, lawyer, author and an authority on water boundaries who retired from the U.S. Coast and Geodetic Survey, died on Oct. 20, 1975 in George Washington University Hospital.

In 1973 the U.S. Board on Geographic Names designated a newly discovered underwater mountain in the northeast Pacific Ocean as the Shalowitz Sea Mount in honor of his "monumental contribution for more than three decades in the realm of the law of the sea, particularly seaward boundaries culminating in 'Shore and Sea Boundaries,' which has become a classic in the field of oceanography, marine cartography and the law of the sea." According to the board, the occasion was the first time that undersea features were named for a living person.

"Shore and Sea Boundaries," Shalowitz's major publication effort, was a 1,200-page work published in two volumes. It has been cited and quoted extensively in legal briefs and court decisions and in 1966 was a basis for the Society of American Military Engineers giving Shalowitz the society's Colbert Medal.

Shalowitz also received a gold medal and citation, the highest award of the Commerce Department, for his "outstanding contributions to science and technology in the field of hydrographic and cartographic engineering."

Shalowitz received his basic engineering training at Baltimore Polytechnic Institute. In 1916 he entered the service of the Coast Survey, first as a commissioned officer in the field, engaged in geodetic, topographic and oceanographic surveys in the United States, Alaska and the Virgin Islands.

Later, as a cartographic engineer in the Washington office, he interpreted marine surveys and nautical charts for legal and scientific use. He retired in 1964 as a special assistant to the director of the Survey.

Shalowitz received his L.LB. degree with honors and a J.D. (juris doctor) from Georgetown University. He also received a master of law degree from George Washington University.

With the introduction of echo sounding for measuring depths at sea, Shalowitz contributed to the design and development of a new type of nautical chart in which submarine features were represented by depth contours rather than by isolated depths. This was a great aid to sea navigation.

He was a member of the Washington Academy of Sciences, the American Technion Society, American Geophysical Union, the American branch of the International Law Association and the American Congress on Surveying and Mapping.

A native of Latvia, Shalowitz came to the United States at the age of 3. He was a member of Ohev Shalom Congregation.

He leaves his wife, Pearl, and a son, Ernest, at home, and another son, Erwin, of Bethesda, and three grandchildren.

JOURNAL OF THE WASHINGTON ACADEMY OF SCIENCES

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